Med.
A

TRANSACTIONS

OF THE

AMERICAN PEDIATRIC SOCIETY

TWENTY-FIRST SESSION

HELD AT THE CURTIS HOTEL, LENOX, MASS., ON MAY 27 AND 28, 1909.

EDITED BY

LINNÆUS EDFORD LA FÉTRA, M.D.

VOLUME XXI.

REPRINTED FROM
ARCHIVES OF PEDIATRICS
1909–1910

1940 63
5.2.25

E. B. TREAT & CO., PUBLISHERS
241–243 WEST 23D STREET, NEW YORK
1910

A. G. SHERWOOD & CO., PRINTERS,

434 LAFAYETTE STREET,

NEW YORK.

CONTENTS.

List of Presidents v

List of Officers for the Year 1909 v

List of Meeting Places v

List of Officers for the Year 1910 vi

List of Members vi

List of Deceased Members viii

Minutes of the Twenty-first Annual Meeting 1

I. Presidential Address. Medical Work in the Charitable Institutions of Boston. By CHARLES P. PUTNAM, M.D. 2

II. The Position and Work of the American Pediatric Society toward Public Questions. By THOMAS MORGAN ROTCH, M.D. 7

III. Discussion by DRS. LA FÉTRA, ADAMS, ABT and KERLEY 28

IV. A Few of the Things a Pediatrician Should Teach. By P. J. EATON, M.D. 40

V. Spasmodic Stricture of the Esophagus. By L. E. LA FÉTRA, M.D. 48

VI. A Case of So-called Congenital Malaria By FRANK S. MEARA, M.D. 55

VII. Some Observations Concerning Scarlet Fever, Rubella Scarlatinosa and So-called Duke's Disease. By D. J. MILTON MILLER, M.D. 63

VIII. The Urine in the Gastrointestinal Diseases of Infancy. An Analysis of the Results of the Examination of the Urine in 300 Cases. By JOHN LOVETT MORSE, M.D., and BRONSON CROTHERS, M.D. 75

IX. The Soy Bean in Infant Feeding. Preliminary Report. By JOHN RUHRÄH, M.D. 80

X. An Abstract of Finkelstein's View of Infantile Alimentary Intoxication. By IRVING M. SNOW, M.D. 92

XI. Investigations upon the Irritating Effects of Cathartic Drugs. By I. A. ABT, M.D. 104

XII. A Case of Cerebral Hemorrhage (Birth), with Operation. By FRANK S. MEARA, M.D., and ALFRED S. TAYLOR, M.D. 114

XIII. Congenital Cyst of the Liver, with Report of a Case. By Henry L. K. Shaw, M.D., and Arthur W. Elting, M.D..... 122

XIV. The So-called Thymus Death, with an Account of 7 Cases of Sudden Death in One Family. By J. P. Crozer Griffith, M.D. 128

XV. A Preliminary Report of the Metabolism of Diphtheria. By John Howland, M.D 141

XVI. Pneumococcic Infection. By Walter Lester Carr, M.D..... 151

XVII. Report of a Case of Long-Standing Empyema, with a Note on Race as a Predisposing Factor in the Recurrence of Empyema. By Alfred Hand, Jr., M.D..... 158

XVIII. A Case of Congenital Stenosis of the Duodenum. By Rowland G. Freeman, M.D..... 164

XIX. The Medical Work of the Juvenile Court of Cook County. By Frank S. Churchill, M.D., and James A. Britton, M.D..... 168

XX. The Stools of the Newborn and their Significance. By Thomas S. Southworth, M.D..... 180

XXI. A Specimen of Formed Meconium Simulating Intestine. By Rowland G. Freeman, M.D..... 192

XXII. The Bacteriology of Acute Infections of the Respiratory Tract in Children, with Especial Reference to Influenza. By L. Emmett Holt, M.D..... 193

PRESIDENTS.

1889.	A. Jacobi, M.D.	1900.	Henry Koplik, M.D.
1890.	J. Lewis Smith, M.D.	1901.	Wm. D. Booker, M.D.
1891.	T. M. Rotch, M.D.	1902.	W. S. Christopher, M.D.
1892.	Wm. Osler, M.D.	1903.	J. P. Crozer Griffith, M.D.
1893.	A. D. Blackader, M.D.	1904.	Augustus Caillé, M.D.
1894.	John M. Keating, M.D.	1905.	C. G. Jennings, M.D.
1895	F. Forchheimer, M.D.	1906.	A. Jacobi, M.D.
1896.	Joseph O'Dwyer, M.D.	1907.	B. K. Rachford, M.D.
1897.	Samuel S. Adams, M.D.	1908.	C. G. Kerley, M.D.
1898.	L. Emmett Holt, M.D.	1909.	Charles P. Putnam, M.D.
1899.	Wm. P. Northrup, M.D.	1910.	David L. Edsall, M.D.

OFFICERS, 1909.

President.....Charles P. Putnam, M.D.
First Vice-President.....Isaac A. Abt, M.D.
Second Vice-President.....Thomas S. Southworth, M.D.
Secretary.....Samuel S. Adams, M.D.
Treasurer.....J. Park West, M.D.*
Recorder and Editor.....L. E. La Fétra, M.D.

COUNCIL.

F. S Churchill, M.D., *Chairman*.

L. Emmett Holt, M.D.	George N. Acker, M.D.
John Lovett Morse, M.D.	R. G. Freeman, M.D.
S McC. Hamill, M.D.	Alfred Hand, Jr., M.D.

MEETING PLACES.

1888.	Washington, D. C. (Organization), September 18.
1889.	Washington and Baltimore, September 20 and 21.
1890.	New York, June 3 and 4.
1891.	Washington, September 22 and 25.
1892.	Boston, May 2, 3 and 4.
1893.	West Point, N. Y., May 24, 25 and 26.
1894.	Washington, May 29 and June 1.
1895.	Virginia Hot Springs, May 27, 28 and 29.
1896.	Montreal, May 25, 26 and 27.
1897.	Washington, May 4, 5 and 6.
1898.	Cincinnati, June 1, 2 and 3.
1899.	Deer Park, June 27, 28 and 29.
1900.	Washington, May 1, 2 and 3.
1901.	Niagara Falls, May 27, 28 and 29.
1902.	Boston, May 26, 27 and 28.
1903.	Washington, May 12, 13 and 14.
1904.	Detroit, May 30, 31 and June 1.
1905.	Lake George, N. Y., June 19, 20 and 21.
1906.	Atlantic City, N. J., May 30, 31 and June 1.
1907.	Washington, May 7, 8 and 9.
1908.	Delaware Water Gap, May 25, 26 and 27.
1909.	Lenox, Mass., May 27 and 28.
1910.	Washington, May 3, 4 and 5.

*Deceased.

OFFICERS, 1910.

President.....David L. Edsall, M.D.
First Vice-President.....D. J. Milton Miller, M.D.
Second Vice-President.....E. W. Saunders, M.D.
Secretary.....Samuel S. Adams, M.D.
Treasurer.....Charles Hunter Dunn, M.D.
Recorder and Editor.....L. E. La Fétra, M.D.

COUNCIL

L. Emmett Holt, M.D., *Chairman*.

John Lovett Morse, M.D. — R. G. Freeman, M.D.
S. McC. Hamill, M.D. — Alfred Hand, Jr., M.D.
George N. Acker, M.D. — L. E. La Fétra, M.D.

MEMBERS.

Abt, Isaac A., M.D.....4326 Vincennes Avenue, Chicago
Acker, George N., M.D.....913, Sixteenth Street, Washington
Adams, Samuel S., M.D.....1, Dupont Circle, Washington
Baines, Allen M., M.D.....228, Bloor Street, Toronto
Blackader, A. D., M.D.....236, Mountain Street, Montreal
Booker, William D., M.D.....208, West Monument Street, Baltimore
Bovaird, David, Jr., M.D.....120, West Fifty-eighth St., New York
Buckingham, E. M., M.D.....346, Marlborough Street, Boston
Caillé, Augustus, M.D.....753, Madison Avenue, New York
Carr, Walter Lester, M.D.....68, West Fifty-first Street, New York
Chapin, Henry Dwight, M.D.....51, West Fifty-first Street, New York
Churchill, F. S., M.D.....439, North State Street, Chicago
Cotton, A. C., M.D.....1485, Jackson Boulevard, Chicago
Cowie, D. M., M.D.....Lawrence Building, Ann Arbor, Mich.
Crandall, Floyd M., M.D.....113, West Ninety-fifth Street, New York
Dorning, John, M.D.....124, West Eighty-first Street, New York
Dunn, Charles Hunter, M.D.....220, Marlborough Street, Boston
Eaton, Percival J., M.D.....131, N. Highland Avenue, E. E., Pittsburgh
Edsall, David L., M.D.....346, South Sixteenth Street, Philadelphia
Fife, Charles A., M.D.....1927, Chestnut Street, Philadelphia
Forchheimer, F., M.D.....Fourth and Sycamore Streets, Cincinnati
Freeman, Rowland G., M.D.....211, West Fifty-seventh Street, New York
Graham, E. E., M.D.....1713, Spruce Street, Philadelphia
Griffith, J. P. Crozer, M.D.....1810, Spruce Street, Philadelphia
Hamill, S. McC., M.D.....1822, Spruce Street, Philadelphia

HAND, ALFRED, JR., M.D. ..... 1724, Pine Street, Philadelphia
HEIMAN, HENRY, M.D ... 50, West One Hundred and Twentieth Street, New York
HOLT, L. EMMETT, M.D. ..... 14, West Fifty-fifth Street, New York
HOWLAND, JOHN, M.D. ..... 49, East Fifty-third Street, New York
HUBER, F., M.D. ..... 200, East Seventeenth Street, New York
JACOBI, A., M.D. ..... 19, East Forty-seventh Street, New York
JENNINGS, CHARLES G., M.D. ..... 457, Jefferson Avenue, Detroit
KERLEY, CHARLES G., M.D. ..... 132, West Eighty-first Street, New York
KNOX, J. H. MASON, M.D. ..... 804, Cathedral Street, Baltimore
KOPLIK, HENRY, M.D. ..... 30, East Sixty-second Street, New York
LADD, MAYNARD, M.D. ..... 295, Beacon Street, Boston
LA FÉTRA, LINNÆUS E., M.D. ..... 110, East Sixty-second Street, New York
MACHELL, H. T., M.D ..... 05, Bellevue Avenue, Toronto
MARTIN, C. F., M.D. ..... 33, Durocher Street, Montreal
MCCOLLOM, JOHN H., M.D. ..... 740, Massachusetts Avenue, Boston
MEARA, FRANK S., M.D. ..... 400, West End Avenue, New York
MILLER, D. J. MILTON, M.D. ..... 1700, Pacific Avenue, Atlantic City, N. J.
MORSE, J. LOVETT, M.D. ..... 70, Bay State Road, Boston
NICOLL, MATTHIAS, M.D. ..... 124, East Sixtieth Street, New York
NORTHRUP, WILLIAM P., M.D. ..... 57, East Seventy-ninth Street, New York
PUTNAM, CHARLES P., M.D. ..... 03, Marlborough Street, Boston
RACHFORD, B. K., M.D. ..... 323, Broadway, Cincinnati
ROTCH, THOMAS MORGAN, M.D. ..... 107, Commonwealth Avenue, Boston
RUHRÄH, JOHN, M.D. ..... 830, North Eutaw Street, Baltimore
SAUNDERS, E. W., M.D. ..... 3003, Lafayette Avenue, St. Louis, Mo.
SHAW, HENRY L. K., M.D. ..... 108, Washington Avenue, Albany
SNOW, IRVING M., M.D. ..... 470, Franklin Street, Buffalo
SOUTHWORTH, THOMAS S., M.D. ..... 807, Madison Avenue, New York
STARR, LOUIS, M.D. ..... 1818, Rittenhouse Square, Philadelphia
TOWNSEND, CHARLES W., M.D. ..... 76, Marlborough Street, Boston
WENTWORTH, A. H., M.D. ..... 352, Marlborough Street, Boston
WESTCOTT, THOMPSON S., M.D. ..... 1833, Spruce Street, Philadelphia
WILLIAMS, HAROLD, M D. ..... 528, Beacon Street, Boston
WINTERS, J. E., M.D. ..... 25, West Thirty-seventh Street, New York

---

HONORARY MEMBERS.

DR. JOHN THOMPSON ..... Edinburgh, Scotland
DR. GEORGE F. STILL ..... London, England
DR. HENRY ASHBY ..... Manchester, England
DR. O. HEUBNER ..... Berlin, Germany
DR. THEODOR ESCHERICH ..... Vienna, Austria
DR. WILLIAM OSLER ..... Oxford, England
DR. A. BAGINSKY ..... Berlin, Germany
DR. V. HUTINEL ..... Paris, France
DR. CHARLES RAUCHFUSS ..... St. Petersburg, Russia

Deceased.

John A. Jeffries, M.D.
Born, September 2, 1859,
Died, March 26, 1892.

Thomas F. Sherman, M.D.
Born, March 17, 1856,
Died, September 26, 1893.

John M. Keating, M.D.
Born, April 20, 1852,
Died, November 17, 1893.

Charles Warrington Earle, M.D.
Born, 1845,
Died, November 19, 1893.

J. Lewis Smith, M.D.
Born, October 15, 1827.
Died, June 9, 1897.

Joseph O'Dwyer, M.D.
Born, October 12, 1841,
Died, January 7, 1898.

John Henry Fruitnight, M.D.
Born, November 9, 1851,
Died, December 18, 1900.

Frederick A. Packard, M.D.
Born, November 17, 1862,
Died, November 1, 1902.

Walter S. Christopher, M.D.
Born, 1859,
Died, March 2, 1905.

Leroy Milton Yale, M.D.
Born, February 12, 1841,
Died, September 12, 1906.

James Park West, M.D.
Born, June 27, 1858,
Died, June 25, 1908.

MINUTES OF THE TWENTY-FIRST ANNUAL MEETING OF THE AMERICAN PEDIATRIC SOCIETY.

Held at the Curtis Hotel, Lenox, Mass., on May 27 and 28, 1909.

FIRST SESSION, MAY 27TH, 10 A.M.

The meeting was called to order by the president, Dr. Charles P. Putnam, of Boston. The following members were present: Drs. Isaac A. Abt, Chicago; George N. Acker, Washington; Samuel S. Adams, Washington; A. D. Blackader, Montreal; Walter Lester Carr, New York; F. S. Churchill, Chicago; Percival J. Eaton, Pittsburg; Rowland G. Freeman, New York; E. E. Graham, Philadelphia; J. P. Crozier Griffith, Philadelphia; S. McC. Hamill, Philadelphia; Alfred Hand, Jr., Philadelphia; L. Emmett Holt, New York; John Howland, New York; Charles G. Kerley, New York; Henry Koplik, New York; L. E. La Fétra, New York; Frank S. Meara, New York; D. J. Milton Miller, Atlantic City; J. Lovett Morse, Philadelphia; Charles P. Putnam, Boston; Thomas Morgan Rotch, Boston; John Ruhräh, Baltimore; E. W. Saunders, St. Louis; Thomas S. Southworth, New York; Henry L. K. Shaw, Albany; J. E. Winters, New York.

There were present as visitors: Drs. Alfred B. Withington, Pittsfield; P. S. Potter, North Adams; Henry Colt, A. L. England, W. A. Wilder, R. W. Paddock and Alfred S. Taylor.

Communications of regret at being absent were received from Drs. Snow, Northrup and Caillé.

The minutes of the twentieth annual meeting were adopted as published in the ARCHIVES OF PEDIATRICS.

The following papers were read:

1. President's Address: "Medical Work in the Charitable Institutions of Boston." By Dr. Charles P. Putnam, Boston.

The President of the Society welcomed the members to Mas-

sachusetts and to Lenox and thanked them for giving him the pleasure of presiding over this meeting of the Society.

He then spoke of the death of Dr. West, who had been for many years the Treasurer of the Society. Dr. West had endeared himself to the members of the Society by his pure and noble character. He had been an ideal practitioner, unconscious of himself but fully alive to all his duties toward his patients and the community.

In his address, the President gave a sketch of changes that had been introduced into the Children's Institutions Department of the City of Boston by a board of unpaid trustees appointed in 1897, of which he had been first secretary and later chairman, and he urged the members to accept positions as trustees in public institutions for children, saying he would prove by his own experience that there was a vast field for the work of a pediatrist in these institutions.

The State's children, whether juvenile offenders, truants or dependent or neglected children, should not be pictured as being on the whole healthy and normal. Unsanitary influences in the home were largely responsible for these children coming under the care of the State. Extra labor was therefore necessary on the part of the State to counteract the bad home influences from which these children had been taken.

The recognition of feeble-mindedness alone and the treatment of it had done much to elevate the general tone of the whole community.

The board of which Dr. Putnam was a member had been appointed to administer the children's institutions, but not with the idea that any tremendous evils were to be reformed. Nevertheless, the trustees had found many defects of far-reaching importance which they had been able to remove entirely or in part, and this in spite of the fact that these defects had been the result of ignorance and carelessness rather than of deliberate misdoing.

The greatest single success of the trustees had been the abolition, accomplished in the first nineteen months, of the Marcella Street Home, which for many years had been a great almshouse for some four hundred children. It was now universally acknowledged that the city was better off without this almshouse than with it and that it had been a great gain to place the children, formerly in the almshouse, in families, where they were brought up as ordinary members of the community.

In all the divisions of the Children's Institutions Department, a large amount of medical work, most of it given gratuitously, had been introduced, including also the care of the eyes, ears and teeth of the children. In the two schools—the Suffolk School and the Parental School—during the eleven years, the number of teeth extracted amounted to 4,538; the number filled to 2,236, and other treatments of various kinds added much to these numbers. Large numbers of spectacles had been supplied with very excellent effect.

The address ended as follows:

"This sketch of what has been done for the dependent children of the city of Boston gives of necessity only a general outline. More details would furnish strong proof in favor of my argument that work of this kind is in demand and that it is incumbent on us to take up our share of it. If my story sounds as if in our department everything had already been done, let me say that to me it seems as if it had only been begun, for the field broadens out the more it is studied, just as does the field of the pathologist. I look forward to seeing my place taken by some one of my colleagues who will develop the subject of the reform of deficient and delinquent children through physical and mental improvement in greater and greater measure."

2. Dr. Thomas Morgan Rotch, "The Position and Work of the American Pediatric Society toward Public Questions."

This paper was discussed as follows:

3. Dr. Linnæus E. La Fétra, "The Need of and the Relation between a Definite Anatomic Development and Child Labor."

4. Dr. Samuel S. Adams, "The Need of and the Relation between a Definite Anatomic Development and the Athletics of Early Life."

5. Dr. Isaac A. Abt, "The Need of and the Relation between a Definite Anatomic Development and the Grading of Kindergartens."

6. Dr. Charles Gilmore Kerley, "The Need of and the Relation between a Definite Anatomic Development and the Grading of Schools."

These papers were discussesd by Drs. Morse, Freeman, Carr, Holt and Rotch.

7. Dr. F. S. Churchill, "A Report on the Medical Work in the Juvenile Court of Chicago."

8. Dr. P. J. Eaton, "A Few of the Things which a Pediatrist Should Teach."

Discussed by Dr. Morse.

9. Drs. Frank A. Meara and Alfred S. Taylor, "A Case of Cerebral Hemorrhage (birth), with Operation."

This paper was discussed by Drs. Taylor, La Fétra, Koplik, Morse, Holt, Miller, Freeman, Churchill, Hamill, Howland, Kerley; and, in closing, by Drs. Taylor and Meara.

10. Dr. E. W. Saunders, "Danger to the New-Born from Gas Heaters."

Discussed by Dr. Adams.

11. Dr. L. E. La Fétra, "Spasmodic Stenosis of the Esophagus; a Case Demonstrated by the X-Ray."

Discussed by Dr. Rotch.

SECOND SESSION, 3 P.M.

12. Dr. L. Emmett Holt, "Influenza—The Part It Plays in Hospital Infections; the Value of Cultures as a Means of Diagnosis; Influenza Carriers."

This paper was discussed by Drs. Rotch, Rowland, Koplik, Abt, Hamill, Southworth, Miller, La Fétra, Shaw, Churchill and Kerley. Dr. Holt closed the discussion.

13. Dr. Walter Lester Carr, "Pneumococcus Infection."

14. Dr. Alfred Hand, Jr., Philadelphia, "Report of a Case of Long-standing Empyema, with a Note on Race as a Predisposing Factor in the Occurrence of Empyema."

This paper was discussed by Drs. Freeman, Adams and Rotch. Dr. Hand closed the discussion.

15. Dr. John Howland, "Preliminary Report of Metabolism of Diphtheria."

This paper was discussed by Dr. Meara.

16. Dr. R. G. Freeman, (*a*) "A Case of Atresia of the Duodenum"; (*b*) "A Specimen of Formed Meconium Simulating Intestine."

Discussed by Dr. Rotch.

17. Dr. Frank S. Meara, "A Case of So-called Congenital Malaria."

THIRD SESSION, 8 P.M.

18. Dr. D. J. M. Miller, "Some Considerations Concerning Scarlet Fever, Rubella Scarlatinosa, and Duke's Disease."

This paper was discussed by Drs. Griffith, Koplik, Shaw, Adams, Freeman, Abt, Southworth, Putnam, Hand and Griffith. Dr. Miller closed the discussion.

19. Dr. J. P. Crozer Griffith, "The So-called Thymus Death, with an Account of 6 Cases of Sudden Death in One Family."

This paper was discussed by Drs. Koplik, Hand, Howland, Meara and Eaton. Dr. Griffith closed the discussion.

FOURTH SESSION, MAY 28TH, 9:30 A.M.

20. Dr. Thomas S. Southworth, "The Stools of the Newborn and their Significance."

This paper was discussed by Drs. Morse, Griffith, Winters and Freeman. Dr. Southworth closed the discussion.

21. Dr. John Lovett Morse, "The Urine in the Gastro-intestinal Diseases of Infancy."

This paper was discussed by Drs. Churchill, Koplik and Miller. Dr. Morse closed the discussion.

22. Dr. Isaac A. Abt, "The Irritating Effects of Cathartic Drugs on the Intestinal Mucosa."

This paper was discussed by Drs. Morse, Southworth, Adams, Blackader and Rotch. Dr. Abt closed the discussion.

23. Dr. E. W. Saunders, "The More Frequent Use of Gavage in Pediatric Practice."

This paper was discussed by Drs. Eaton, Rotch, Miller, Koplik and Blackader. Dr. Saunders closed the discussion.

24. Dr. John Ruhräh, "Soy Beans in Infant Feeding; Preliminary Report."

25. Dr. Henry L. K. Shaw, "Congenital Cysts of the Liver."

The following papers were read by title:—

"Traumatic Pneumothorax, with Radiographic Studies," by Dr. Francis Huber and Dr. I. S. Hirsch.

"An Abstract of Finkelstein's Views on Alimentary Intoxication and Atrophy," by Dr. Irving M. Snow.

EXECUTIVE SESSION, MAY 28TH, 10:30 A.M.

The report of the Council was read by Dr. Churchill and its recommendations adopted, as follows:—

As officers for the ensuing year there were elected:—

President, Dr. David L. Edsall, Philadelphia.

First Vice-President, Dr. D. J. Milton Miller, Atlantic City.

Second Vice-President, Dr. E. W. Saunders, St. Louis, Mo.

Secretary, Dr. Samuel S. Adams, Washington.

Treasurer, Dr. Charles Hunter Dunn, Boston.

Recorder, Dr. L. E. La Fétra, New York.

As new member of the council, Dr. L. E. La Fétra.

The resignation of Dr. Wilson was accepted. Dr. C. F. Martin was reinstated.

Elected to membership, Dr. D. M. Cowie, Ann Harbor, Mich.; Dr. Henry Heiman, New York; Dr. H. T. Machell, Toronto.

As member of the Executive Committee, Congress of American Physicians and Surgeons, Dr. S. S. Adams.

The report of the Treasurer was audited and reported correct by Dr. Hand.

It was recommended that in the publication of papers preference be given to those showing original research.

The annual assessment for dues was made $10.

The time and place for the next annual meeting were set for Washington, May 3, 4 and 5, 1910.

SAMUEL S. ADAMS, M.D.,
Secretary.

L. E. LA FÉTRA, M.D.,
Recorder.

THE POSITION AND WORK OF THE AMERICAN PEDIATRIC SOCIETY TOWARD PUBLIC QUESTIONS.

BY THOMAS MORGAN ROTCH, M.D.,
Boston.

At a meeting held for the discussion of the child labor question, under the auspices of Miss Gurnee at the Colony Club in New York, on January 4, 1909, a number of representative members of the Pediatric Society were present. Through the endorsement which they gave to my views I have been encouraged to bring the relation which our Society should, in my opinion, take on public questions in general before you as a whole.

The audience at the New York meeting included a number of different interests, each working for the supposed welfare of the children of our country, but each determining its course of action from its own particular point of view, the point of view being regulated by the especial work which the especial interest was engaged in.

A month later, in February, I again addressed a large and still more representative audience in the Auditorium in Chicago. At that meeting I learned from representative individuals connected with the most diverse interests what a heterogeneous mass of unclassified and unadjusted ideas and opinions, not only we physicians are confronted with in approaching the question of the safeguarding of early life, but how the same difficulties baffle the legal profession both in correcting bad laws and in enforcing good ones.

In both New York and Chicago I tried to impress upon educators and philanthropists that no one set of workers could know all sides of the problems connected with early life and that the intelligent solution of these problems could only be accomplished by making use of the combined knowledge of each student in his own particular province of research. I explained to them that what many of these questions needed for practical consideration

was not merely the general knowledge of children possessed by the rank and file of the medical practitioners, even though as individuals they might be eminent in some other branch of medicine than Pediatrics.

I tried to make them understand that the mature knowledge derived from a special study of years in connection with early life was required practically in the solution of these problems. I told them that our Pediatric Society provided such a body of men whose knowledge ought to be utilized if they expected to deal with these problems intelligently, and not by their ignorance of the vital principles connected with childhood defeat their own good intentions and their own excellent philanthropic work.

The Chicago meeting was essentially a Child Labor Convention, and the whole force of the Child Labor Committee was centered on influencing Congress to carry out the reforms considered necessary by the Child Labor philanthropists to ameliorate the undoubted wrongs which the young people of our country are enduring.

Through the endeavors of the Child Labor Movement a bureau for children has been established as a part of the Department of the Interior, and it is to this bureau that in the future we must turn to bring about the reforms which are so much needed all over the country.

The large body of women who are connected with the Child Labor Movement has added greatly to the accomplishment of markedly successful results, and too much praise cannot be given to the whole number of both men and women who now for some years have labored unceasingly.

We physicians, however, whose mission it really is to guide the progress of the various reforms connected with early life and see that they do not go astray, should interest ourselves in curbing exaggerated ideas and in the prevention of unwisely pressing upon our legislators unsound views on which to base new laws.

For instance, one of the main objects for which the Child Labor Committee has been striving is to obtain a law throughout the country which shall increase the age in years required before a child shall labor, notably in the mills. Not only would such a law, if it could be obtained (which it never can be), be practically unwise and essentially wrong, but it would, according to our

knowledge of young human beings, probably do harm in many ways.

It is well known that children vary in their development, not only in different parts of our country, but that this variation occurs in different races and notably in different families of the same race.

To pass a universal law, therefore, that demands fourteen years of life before a child can work, inevitably means that children in one part of the country or of an especial race or parentage will be made to work too soon or in many cases not soon enough.

It is true that the age law for labor in many of our states is undoubtedly too low, as for instance that of ten years in South Carolina, but for that reason only we should not insist on a law of fifteen or sixteen years which in some states and in some classes would be unwise and unpractical. When through the efforts of the Child Labor enthusiasts the age limit in the District of Columbia was increased from twelve to fourteen years a significant result followed. Strong, well-developed negro boys, twelve and thirteen years of age, who had been working without harm to themselves or to anyone else, were cut off from their work and became loafers on the streets. The corollary to this, however, was experienced in numbers of boys of fourteen who although from their general lack of development were unfitted for many different kinds of work, yet were employed under the new law to do work which they were manifestly unfitted to do.

To cut off the cotton pickers in the South from doing the work which they are best fitted for, because they are not fourteen or fifteen years of age, would be manifestly unwise, and, in fact, knowing the predisposition to tuberculosis which exists among this class, we should deprive them of the very out-door work which is best for them.

On the same principle the grading of young children in athletic contests, in kindergartens and in schools according to the number of years since they have been born is manifestly wrong. Chronologic age varies to such a degree that groups of individual children of the same chronologic age present many radically different degrees of strength for labor; for resisting the different strains brought to bear on their physical and mental development in the different grades of kindergarten and school and in the various physical contests which naturally come into their school

life. The present state of the efforts at reform among philanthropists, Child Labor enthusiasts, educators, and those who are interested in athletic sports is somewhat chaotic. What is needed to organize the real power of the people for good in all these philanthropic endeavors is to persuade these different classes of workers to listen to what we can tell them of the actual facts which govern the true solution of the child problem, facts which are best known and enunciated by those who have made this problem their life work.

I was much pleased with the message which came to me from certain legislators in Washington where I was about to read a paper on the developmental questions of childhood, that what Congress needed when the question of Child Labor was vigorously and insistently pushed upon it by the National Child Labor Committee, was to hear from the medical profession the actual facts as to the ability of children to do without harm certain kinds of work. They impressed upon me that enthusiastic philanthropists were often unsafe advisers and advocates. They also advised me personally not to identify myself too closely with any one group of philanthropists but to simply state practical scientific facts.

The power to work physically and the degree of resisting power needed in kindergartens, schools and athletics cannot be estimated by years and therefore rules and laws attempting to so classify are illusive and wrong. Beyond this, however, even if chronologic laws had been proved to be good, they would fail to accomplish the desired results by the impossibility of carrying them out.

The lawyers acknowledge that they are practically helpless in their endeavor to enforce these laws because they are so easily evaded. The judges, especially of the juvenile criminal courts, tell me that a remarkable degree of perjury as to age exists in the courts. Thousands of false returns of age are given, false birth certificates are exceedingly common, and over and over again no birth certificates can be found.

In some states, again, illegitimate births are purposely not recorded. The consequence is that although the law of an especial state says that no child under fourteen years of age shall work in a mill, the mills are full of children only twelve or thirteen years of age. This happens not because the mill owners allow these children to enter their employ, for they actually employ physicians to try to tell them how old the children are in order

to obey the law. The trouble comes from the children's parents or guardians, who swear that their child's age conforms to the especial law of the state in which it lives.

In addition to this it is now well known that height, weight, and the eruption of the teeth are exceedingly delusive. A child of eleven may look, so far as height and weight are concerned, as though he were twelve or twelve and a half years of age, or again as though he were ten or nine and a half years of age. Careful investigations have shown, in fact, that height may vary by a year and a half and that weight is equally illusive.

The one great principle, which I suppose we all as pediatricians will accede to, is that a child should be entered and graded in kindergarten when his physical development shows that he has sufficient resisting power to normally cope with whatever strains, whether mental or physical, he has to encounter in those grades.

In like manner that when without mental or physical fatigue he can carry out the curriculum in certain grades in school he should be placed in those grades. Again that an exact examination of his physical development should be made, no matter what his chronologic age, before he be allowed to contest in athletics, and then only after he has been grouped with an equal stage of development and not chronologically.

Finally that as physicians we should so study the different degrees of force-outcome needed for different employments as to gauge the degree of physical development required for that employment.

With a physical developmental index we can well dismiss laws for chronologic age and in this way also not be led astray by idiosyncrasy of race, family or individual. Each child by this method can be judged by its individual development and placed, whether in school, athletics, or work, where it belongs physically, and where it can live its early life in competition with other individuals, whose development being the same will give opportunity for normal unstrained growth.

It is worthy of note that Professor Pryor, of the University of Kentucky, has already shown that there exists a decided difference in the physical development of the osseous centers of males and of females; the latter being decidedly in advance of the former even from birth. This again gives us a reason for discarding chronologic age as an index for classification but still enunciates

the fact that although girls develop early and as we know become mature some years sooner than do boys, yet that we should determine their lives by their individual development.

Following this rule in some cases girls should be classified sometimes with boys who are older, or again with boys who are younger or of an equal age. The entire question depends on whether they show equal, less, or greater development.

If you take into consideration what I have said really means, you will, I am sure, agree with me that the determination of degrees of development to correspond with the many different conditions and requirements of child life belongs to a body of physicians, trained for this especial work and not to the laity. The laity, however earnest and enthusiastic it may be, is in no way fitted to deal with what is really an intricate scientific study combining a knowledge of anatomy, physiology, hygiene, years of intelligent and trained observation of children in health and in disease and the continual meeting as we do in our consulting rooms with children who are suffering from unwise rules and bad laws.

It will not be an easy matter to make the philanthropic laity see and acknowledge this, but it is just as wrong and undignified for us who are supposed to regulate the health and well-being of children to stand by and to allow the ignorant though humane laity to take charge of this duty to the young, as it was for us to allow for so many years the regulation of infant feeding to remain in the hands of the proprietors of patent foods.

As I look at it, it is the province of this army of self-sacrificing, indomitable and philanthropic women and laymen-captains who are doing such a great work for humanity all over our country to attack the mills, the sweat-shops and the many criminal strongholds where children's lives are being ruined and to drag this crime into the light of day. When once, however, this work is done, they should submit to the guidance of the generals who possess the necessary knowledge for best dealing with the situation. Then when the question of where the reform is needed and the reasons for it are scientifically settled the legal profession should be consulted as to how these reforms can be carried out and how new and better laws can be framed.

The legislators should then be informed of their manifest duty to frame laws, untrammelled by the importunities of the enthusiastic laymen whose arguments and methods are too often

unwittingly handicapped by sentiment. More than this is the tendency of the non-medical workers of the present age to deal with and to think that they can solve abstruse medical questions without expert advice.

In the question of athletics we shall probably be met more than half way by those who are interested in protecting our boys and girls from over physical strain. Already since stating my views on an anatomic index of development, as an aid in controlling athletic sports, at the Washington meeting, the Secretary of the Navy has sent for information on this subject, stating that he thinks it may be useful when brought to bear on the boys at the United States Naval Academy.

While it is fairly evident what the position of the Pediatric Society should be in regard to Child Labor and perhaps equally evident to us as pediatricians what our position should be in regard to kindergartens and to schools, yet we have here a more complex problem to deal with and we shall probably be looked upon by the educators still more askance and with still greater disbelief than by the Child Labor devotees. In my mind, however, there is no question but that a radical reform is needed in the organization, especially of the schools.

It will not be quite so difficult a task with the kindergartens as more or less dissatisfaction in regard to them has arisen among the laity themselves.

The schools present a problem in themselves; a problem made still more difficult from the fact that the recognized reformers trusted by the laity are as a rule non-medical men. It is true that the aid of physicians is called in and in certain lines is most useful, but the specialists are those connected with the eye, the ear, the throat, and the hygienist, and it is seldom that a professed pediatrician is called in council. Yet the professional laboratories (the consulting rooms of the pediatricians), are where the most knowledge concerning unwise school regulations can be obtained.

The educators are too much inclined to take it for granted that they know what is best for children. They seem to be unaware that there is a still further and more advanced knowledge of the finer factors of the child educational problem beyond what they receive from the general medical practitioner or from the reading of books on school hygiene.

We pediatricians, however, know from personal observation of the many instances where evil results arise from the present

supposed-to-be most enlightened organization of the schools.

It is not by any means the fault always of the school teachers. This class of the laity is long suffering, hemmed in and overborne on the one hand by unenlightened and practically blind parents, and on the other by would-be educational authorities.

The school teachers see, as do the physicians, the evil results of demanding too much of the bright, precocious brains. They know that advancing such brains to a higher grade when not upheld by a proper physical development is productive of future, and sometimes irreparable harm, and the presentation in the physicians' consulting rooms of broken down precocious anomalies.

The teacher protests, the physician reproves and advises, but the parents insist in upholding their mistaken prerogatives from false pride based upon ignorance. Only lately some prominent school teachers have expressed to me their disgust at the discussion at an educators' meeting where they went to learn. The educators in taking up the question of what studies should be taught in the various grades in the schools dwelt entirely on what they as individuals preferred to teach rather than on what studies were best suited to the especial development of the brain which was to undertake them. They overlooked entirely that the study must be adapted to the brain power rather than the brain at random to the study.

A marked and significant example of how the great schools in England, and to a somewhat less extent in this country, have for centuries overlooked the physical laws which are so evident in the young, arises in the question of sleep, and its relation to the growing brain and to the individual development. Tradition has allowed these schools for years to make little distinction between the younger boys and the older boys as regards the number of hours set aside for sleep.

The well-known physiological fact that the earlier the development the longer the sleeping period required has been and still is disregarded. One of our colleagues, Dr. Acland, of London, has sounded the alarm at this lack of a practical application of the laws of development, and it is encouraging to note that English educators have taken notice of his wise observations.

With these evident facts before us it would seem almost the duty of the American Pediatric Society to use the great, wise and efficient influence which it surely is well fitted to exert, in assisting in the work of safeguarding early life by assuming an en-

tirely independent position in the various communities where individual influence is felt and respected.

We should make ourselves felt in the light of an advisory body, which from time to time shall bring up among the other subjects for discussion at our yearly meetings, one by one, the many important questions connected with child life. We should not, however, allow ourselves to become identified with any one set of workers. We must not only have our peculiar place in the community, but not allow ourselves to be dominated by any of the great divisions which are fighting for children's rights, whether in kindergarten, school, athletics or Child Labor.

What is still more important is that we should, if necessary, actually oppose educators and Child Labor Committees. Knowing that the law for chronologic age which I have spoken of as having lately been passed for the District of Columbia, and which was looked upon with such pride by the Child Labor Committee, is manifestly unwise and retrogressive, we should inform the legislators that we, as pediatrists, in no way subscribe to such laws.

An example of what has already happened is a personal episode which occurred last autumn. The Child Labor Committee asked me to present my paper on the anatomic index at Chicago. They, then, while admitting that my position was sound and would be of great value in aiding their work, also said that they were afraid to have it appear in their transactions as it might interfere with the legislation which they wished to carry out. This was fortunate for me and for us as pediatricians, since if it had appeared it would have identified me with the Child Labor Committee which from my point of view would be most undesirable.

On the other hand I have lately, after spending an hour in explaining the value of an anatomic index to one of the most advanced educators in this country and a member of the National Child Labor Committee, had the opinion expressed by him that if such an index were reliable it would be of inestimable value to the work on Child Labor and would at once settle questions for the committee which have seemed almost insurmountable.

I have already published some work which I have been engaged in for the last four years, and which will, I hope, aid in the establishment of better laws for children.

The principle involved is that we should find some ready means for deciding in every phase of a child's life whether the child is ready physically to enter upon some new and progressive

tax on its mental and physical condition. I believe that such a principle can be sustained by the use of the Roentgen ray as applied to the bones with far greater accuracy than by any other method which up to the present time has been employed.

The various other means in the light of modern investigations are, as I have said, unreliable. Crampton's fine work on the correlation of height and weight with chronologic age shows that such correlation when taken at random may vary as much as a year and a half. His physiologic work in connection with the latter part of childhood is valuable, but is not practical in its application. There is, however, a strong correlation in height, weight and chronologic age when we take them in comparison with groups of an equal development.

This means that an index founded on an anatomic basis is what we should turn to in our endeavor to begin the work of rehabilitation of the rules and the laws for early life. We must first recognize that age does not practically mean only years and months; on the contrary, it means the different stages of use and growth of the functions of early life shown in physiologic laws, and again in the normal progressive development in the growth of the ossific centers. We should, therefore, speak of chronologic age, physiologic age and anatomic age. Each has its place in determining the grading of young human beings from birth to adolescence, and indeed to that period when the ossification of the epiphyses is completed.

The bones represent the steel framework on which rests the building, gives it its real strength, and portrays what its strength can be. The epiphyses are the chief factors in this framework and the keystones of its completed development. They are not only the centers which denote the phases of life, both mental and physical, and show as landmarks when the time has come for more extended use, but they are well known to be important nidi where disease enters and from which it may be carried elsewhere.

This assuming the anatomic development, however, to be of primary importance as an index of development and as a guide for the grading of early life does not mean that we should not also make use of the other evidences of anatomic and physiologic growth as evidence of health, just as the roofing and completion of the building is a large part of representing a good building.

A normal condition means healthy organs and muscles as well

as a normal osseous system, and where there is evidence that these former are abnormal we must use such evidence in deciding what rule to follow in a special case. On the other hand, while the muscular system when weak can, as a rule, be strengthened and while a diseased organ can be detected and thus stamp the individual as abnormal, it is in the epiphyses that we look for the groundwork of strength and for the real evidence of normal development. The bones, however, when normal, should show such normality in their quality as well as in their size, and a study of such quality should be made to supplement other evidence of their strength.

It would be out of place here to describe the laborious details of the work which had to be gone through with in order to arrive at what seem to me to be anatomically correct data.

The information which has been obtained of living anatomy, especially of the epiphyses, since the Roentgen method has been employed in investigation, is altering our previous supposed anatomic knowledge to a surprising degree.

Some of the views of anatomists are, through this supplanting of dissections of the dead by actual pictures of the living, being proved to be wrong. It is, therefore, important to obtain, if we can, the opinion and knowledge of those scientists who are fitted both as anatomists and Roentgenologists to criticise such work as I am presenting to you.

Fortunately for me such a scientist has presented himself in the last year and has been supplying me with fresh facts and with expert encouragement. Unknown to me as I was to him, Dr. J. W. Pryor, Professor of Anatomy and Physiology of the State University of Kentucky, and evidently a skilled Roentgenologist, has been carrying on an expert investigation of the development of the joints in exactly the same way as I had been doing. Professor Pryor writes me that he absolutely endorses my work and my views, that he believes that the principle of an anatomic index which I have endorsed is correct and that I am right in saying that in the vast majority of cases the normal and abnormal development of the carpal bones and the epiphyses of the radius and ulna represent a development corresponding to the other joints and their epiphyses. Also that the centers of ossification of the wrist appear and follow each other in such regular developmental succession that they can fairly represent grades of development from birth to fourteen or fifteen years. There is only

one small point on which our anatomic work differs and that is that while Professor Pryor found that the trapezium was the seventh ossific center to appear, I found that this center was represented by the trapezoid. It is possible that Professor Pryor is right, but the difference, so far as the principle of an anatomic index is concerned, is insignificant in carrying out its practical application for the grading of children. The number and massing of the bones is, for my purpose, much more important than their especial order.

My own Roentgenographs of the order of development of the bones of the wrist arranged alphabetically have already appeared in the Transactions of the Association of American Physicians for 1909. These representative developmental divisions can be fairly used to designate groups in such a way that by taking children of the same degree of development we can grade them in kindergarten, school, athletics and in different kinds of manual labor, without distinction as to whether they are boys or girls, of one race or of another, of one family or another, or whether they are sick or well.

In addition to a few of my own pictures I am presenting several which have been given to me by Professor Pryor. These pictures show how in the same family the girl's development is earlier than that of her brother, and then again how in different families the development of the bones differs.

Plate I. is the hand of a girl six years of age, showing the usually found earlier development of the girl over that of her brother, shown in Plate II., whose age when taken was six and one-half years. Plate III. illustrates how the development of the bones differs in different families. This hand is that of a girl three and one-half years of age, and yet it presents just about the same development as that shown in Plate I., a girl six years of age.

Plate IV. represents the development of the wrist which corresponds to the kindergarten period of life. Plates V. and VI. correspond to the school periods. Plate V. represents the hand of a girl five and one-half years of age, and Plate VI. that of a girl eight and one-fourth years of age. Plate VII. corresponds to the Child Labor period and represents the hand of a boy thirteen and one-half years of age.

The plates in this paper, excepting the three given to me by Professor Pryor, were taken from the Roentgen records of the Children's Hospital.

PLATE I.

Girl, age 6 years, sister of boy, age 6½ years, Plate II.

PLATE II.

Boy, age 6½ years, brother of girl, age 6 years. Plate I.

PLATE III.

Girl, age 3½ years. Development about the same as girl, age 6 years, and boy, age 6½ years.

PLATE IV.

Girl, age 3½ years. Represents the development of the Kindergarten Period.

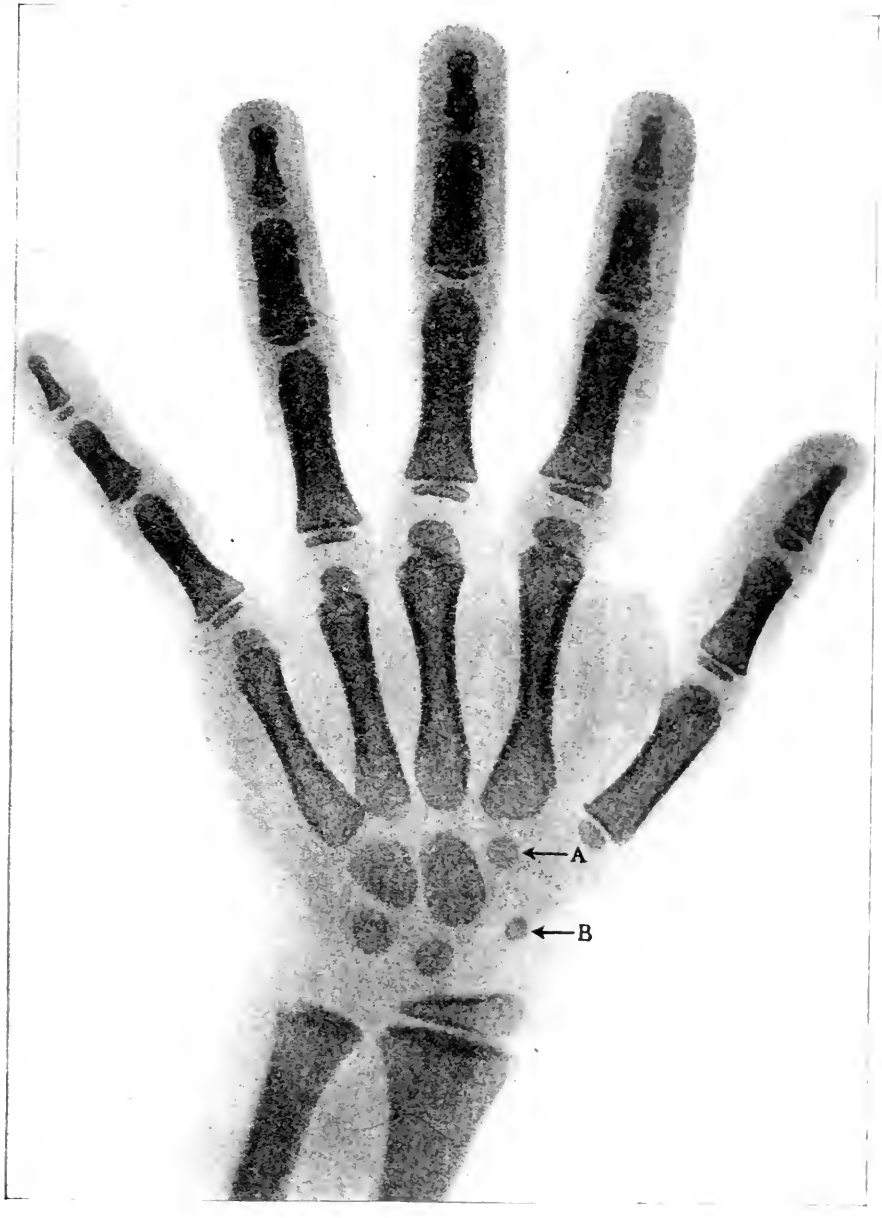


PLATE V.

Girl, age 5½ years. Represents the development of the early School Period.

PLATE VI.

Girl, age 8^{1}_{4} years. Represents the development of a later School Period.

PLATE VII.

Boy, age $13\frac{1}{2}$ years. Represents the development usually existing in the Child Labor Period of a number of the States in America.

For the convenience of those who have not read my previous communications on this subject I offer the following summary:

SUMMARY.

1. There is a manifest need for some developmental index by which physicians acting as an advisory council to the people shall be able to determine the fitness for school and for the physical work of the early years of life.

2. The means formerly used for this purpose are inadequate, whether they may be by height, weight, teeth, statements of parents or guardians or birth certificates.

3. The physiologic test by the pubic hair as worked out by Crampton is an exceedingly valuable contribution, and if, as is possible, it correlates with a more practical anatomic index will aid in deciding in doubtful cases. It evidently, however, from its very nature is not a test which should be widely used in the schools or courts, and it only covers a comparatively short period of life and is one which is not applicable to many questions connected with early and middle school life.

4. Physiologic conditions will probably be found to correspond to anatomic, and great credit should be awarded to Crampton for his suggestions and work directed to the future discarding of chronologic age as the most important guide in solving the problems of early life.

5. Height and weight have long been known to be very inadequate for determining chronologic age and have been conclusively shown to be so by Crampton.

6. Pryor has shown conclusively, and my observations uphold his, that there is a marked difference in the anatomic development of children according to sex and family.

7. The consensus of opinion among odontologists is that the eruption of the teeth as an index of age is illusive and very unreliable.

8. The skeleton represents an illustrative steel framework of development on which the body is built, and this development, when determined, presents the best source from which to evolve an anatomic index for practical use for the safeguarding of early life.

9. The most important part of the skeleton for use as an index lies in the joints.

10. In early life the most important part of the joints is rep-

resented by the epiphyses which form the vital parts connected with the growth in height, as centers for infection and for the dissemination of disease.

11. The carpal bones and the lower epiphyses of the radius and ulna represent the other joints to such a degree in so many instances, and are so much more in evidence and so readily interpreted by the Roentgen method that they can be practically used as an index of development representing the entire bony framework.

12. The best way to use the wrist as an anatomic index is to empyrically classify the carpal bones and the lower epiphyses of the radius and ulna alphabetically in groups.

13. In addition to showing the degree of development these groups signify degrees of possible strength on the ground that the larger the number of bones the greater the strength.

14. The educators and the school teachers should, after advising with the pediatricians, determine what degree of physical and mental development is required for the different grades of kindergarten and school and preliminarily place in those grades the groups of children who show the anatomic index which corresponds to what those especial grades require.

15. In like manner the requirements of certain physical exercises, whether in sports or child labor, should be determined in order that the especial expenditure of physical and nervous force needed for such exercises can be fitted to the especial child.

16. In regard to questions such as Child Labor the philanthropists and those who are especially working for the betterment of child life should withdraw children from unfit labor and obtain information of the especial child's needs from the physicians in accordance with race, family and other conditions of life. The physicians should also determine in what group the especial child belongs, physically and mentally, and state which work it is fitted for.

17. Under the physicians' advice the lawyers should frame sensible and practical laws on a developmental basis, and the legislators should enact such laws whether for the State or for the country at large.

18. While chronologic age in legal questions must be recognized, yet for practical reasons and for the safeguarding of early life we must pay especial attention to the other two types of ages, the physiologic and the anatomic.

19. Finally we must not be misled by thinking that the development of the epiphyses always means actual general physical strength, such as may be found in an individual as for instance by using the Ergograph (R. G. Freeman, *American Journal of the Medical Sciences*, November, 1908). The muscles may be weak and the grasp of the hand may not be up to what may usually be considered the standard, but such a condition should be considered to be abnormal and the individual child should be treated as a sick child and not be classified by the degree of strength which it shows. On the other hand, the carpal bones and the epiphyses of the radius and ulna indicate what the development really is and has been, and the muscular strength and endurance should then be worked up to the degree of development shown by this anatomic index. The individual should be classified just as is done when there is an over-bright brain and a corresponding lack of physical development, and should be relieved from study and improved physically until its anatomic index equals its brain capacity.

The bones of the wrist, therefore, unfold to us an underlying evidence of development which can be used practically for grading in early life, and this Anatomic Index can be disclosed during life and without harm to the individual by means of the Roentgen ray.

DISCUSSION.

THE NEED OF, AND THE RELATION BETWEEN, A DEFINITE ANATOMIC DEVELOPMENT AND CHILD LABOR.

Dr. L. E. La Fétra.—With the general position of Dr. Rotch's paper I am in hearty accord. I have the feeling that, occupying the position it does, the American Pediatric Society should in some way take the lead in devising measures for the protection of children. The plan which Dr. Rotch proposes, that the Society should each year take up some topic with regard to the safeguarding of child life, seems to me an admirable one. The publication of such discussions by this Society would call the attention of the medical journals, and of the lay press as well, to various important topics which might well be the subject of our Society's attention.

It would seem that the chief difficulties in safeguarding child life are not those of ignorance, but the nonenforcement of existing laws in those states where the laws are good and the absence of a public sentiment demanding good laws in many other

states. The part which physicians can take is largely the creation of public sentiment, first for the enforcement of existing laws, and, second, for the enactment of better laws in special states and to overcome particular conditions. For various parts of the country the problems and difficulties are different. In the matter of child labor, for instance, we have the problem in New York State of the children that are overworked during school vacations, especially in the canneries in the country. We also have the problem of the little children, some of whom are under school age, who are overworked in tenements that are licensed as home factories. These cannot be reached by existing laws.

There are certain crying evils that have been very satisfactorily attacked by many of the states throughout our country, but one of the great difficulties is the lack, in the states, of uniformity of legal requirement. Dr. Rotch's paper upon a method of determining the age of children indicates one of the problems continually arising where parents induce their children or the children themselves wish to engage in gainful occupation before they are really able to undertake it. This Society might very well make it a subject of investigation and discussion, "What is the minimum physical development necessary for a child to undertake various kinds of work?" The hours and conditions of such labor should also be investigated.

There are various other problems which might be studied. Some of these have been partly worked out. For example, the prevention of blindness in the newborn. In New York City the regulation of the practice of midwives has recently been very satisfactory, and it is believed that comparatively few cases arise from that source at the present time in New York City. There are many states and cities, however, in which there are either no, or inadequate, regulations, and in the last report of the New York Association for the Blind there is ample evidence that the medical profession could do much throughout the country to prevent the development of ophthalmia neonatorum.

During 1907 there were admitted to the ten principal schools for the blind in the United States and Canada 242 new pupils; of these 59, or 25 per cent., one-quarter were blind because of ophthalmia neonatorum. As we all know, this ought to have been prevented in almost every case; and the blindness of these 59 children is, therefore, needless. In the United States it is estimated that there are between 6,000 and 7,000 persons totally blind from ophthalmia because the well-known Crédé's prophylaxis was not used at time of birth.

In New York State there are only three counties outside of New York that have legally constituted Boards of Examiners for midwives. Rochester now has a Board of Examiners. Notwithstanding these Boards and their regulations, the practice of these midwives, even where licensing is required, is far from what it should be. Miss Crowell's report in 1907, after visiting 500

midwives in New York City, stated that of 303 obstetrical bags inspected only 34 were clean enough to be regarded as first class. The regulations themselves are good, but there is no supervision nor correction unless something wrong happens. Then the investigation begins and the midwife's license may be revoked—after the damage is done. And yet in Buffalo and New York nearly one-half the total number of births in 1905 occurred under care of midwives. Fortunately the American Medical Association, by its Committee on Ophthalmia Neonatorum, has taken up the subject and very satisfactory progress has been made in the way of making recommendations for the more general regulation of midwives, the issuance of circulars of instruction and the distribution of bulbs of 1 per cent. silver nitrate. A good recommendation is that the birth certificate shall be filed within twenty-four hours, and on the certificate it shall be stated what preventive of ophthalmia neonatorum was used; also, if none, why not.

Prevention of the spread of disease in schools has been advanced greatly by medical school inspection. But much remains to be done. In the day nurseries of New York 4,000 children are on an average cared for daily. It is especially in day nurseries that young children are liable to contract disease, so that more careful regulation is needed. In particular, there should be a ban upon cases of vaginitis; for no matter how innocent and simple these may appear, the gonococcus is found in the majority of all cases examined. The diapers taken from all the children should likewise be sterilized and should not be mingled with others in a common tub until they have been boiled. Again, overcrowding is to be prohibited. The minimum air space per crib, according to Dr. Darlington, should be 230 cubic feet instead of the old requirement of 100 cubic feet called for in the New York Board of Health rules and regulations. In many cities there is no regulation whatever nor any medical supervision of day nurseries. It is really a serious question whether children with vulvovaginitis should be allowed to attend public schools. In the regulations for our gonococcus patients at the Vanderbilt Clinic we forbid, though without authority, the children from going to school. The use of the general closets by such children should certainly be prohibited.

In the school life of children there are certain problems which call for investigation. First, the number of hours of work; next, the matter of the midday meal. The curriculum for girls should certainly include, perhaps at first as an elective class, something about the care of young children. "Little mothers'" classes for summer vacation schools already established in New York are a great success. More knowledge of household economy, of cooking and the care of food should be given to girls; this is prophylaxis of the best kind. These girls have the care a large part of the day of the baby or younger children in the family, and

if properly taught they instruct the mother and prevent many errors in hygiene. It is thus possible to reach foreign mothers more satisfactorily through the girls who understand the lectures than by talking directly to the mothers themselves. Six physicians from the New York Department of Health give lectures to mothers and older girls at the various playgrounds and vacation schools. One incident of last summer is worth repeating. It brought home vividly the large part these children have in the care of the tenement babies. "According to instructions, one of the doctors was lecturing to a group of mothers whom he had gathered on a recreation pier. During the talk he noticed a small girl who dragged after her a most reluctant older woman. She held that woman where she could hear the lecture, literally by force of arms. At the end she turned and faced the woman triumphantly: 'There, didn't I tell youse that the doctor says y' mustn't feed no more pickles to the baby?'"

For boys, not only manual, but actual industrial or vocational training should be given, and this despite the opposition of the labor unions. Such instruction enables the boys to earn more when they do work, so that fewer hours of labor are necessary to earn what is needed. Moreover, it fits them for later usefulness. At bottom nearly all sociologic difficulties are a matter of dollars and cents; we can assert that they are all due either to poverty or to ignorance. The school should fit the child to earn his living while it is giving him instructions how to live.

Playgrounds, parks and school gardens are necessary factors in the proper development of the city child, and the medical profession should make its influence felt toward securing and enlarging these beneficent agencies. Quite recently in New York the attempt has been made by two worthy organizations to appropriate a part of two of our parks. Fortunately the attempt was not successful.

The subject of child labor is a large one and the bad effects of prolonged work, work at night or work by young children, are too obvious to need comment. The efforts of the various leagues and committees are having a marked influence in stirring public sentiment against this evil. The keystone of the child labor problem at present is the age limit and compulsory school attendance up to that age. There is general agreement that the lowest age limit for employment should be fourteen years. The New York laws, which are the most stringent, require not only documentary proof of age, but that the child, before receiving working papers, shall present evidence that he has had a school course of not less than eight years.

Documentary proof of age is not required in fourteen states, four territories and in the District of Columbia. This puts a premium upon perjury by parents or guardians. Moreover, deception and evasion of factory inspectors is frequently practised by foremen. It is manifest that the only reason for any age

limit is to make sure the physiologic fitness of the child. It would therefore seem that some more direct physiologic or anatomic standard would be not only capable of easier enforcement, but that it would go more directly to the heart of the matter and prevent the employment of undersized or weakly children.

In the last report of the Consumers' League on the subject of Child Labor Legislation, there is a brief discussion of what should constitute an effective child labor law. Among other prohibitions is this: that no child who is less than 60 inches in height or less than 80 pounds in weight shall be employed. These figures represent about the average height for a boy of fourteen, but the weight average for a boy of twelve and one-half. Surely it would seem best to raise this standard of weight to at least 90 pounds, which is the weight for a boy or girl at about thirteen years. It is a question whether the weight standard for girls ought not to be raised to 100 pounds. This is the average for girls of fourteen years. It is usually about this time that menstruation is established, and overwork, especially long standing during its early and irregular functioning, undoubtedly lays the foundation in many cases for invalidism or sterility. Dr. E. A. Tucker, long resident physician at the Sloane Maternity Hospital, told me that it was his experience that shop and factory girls are very apt to have the infantile type of cervix, with undeveloped uterus, and it was his opinion that this was due to standing too much in early menstrual life.

Unfortunately, on account of differences in the growth of different races (Scotch, Russians and Japanese for example), these height and weight standards do not prove a satisfactory index to the capacity for work. It is exceedingly desirable that some easily applied physical test or standard be discovered. If we are to protect our greatest national asset we must safeguard our boys and girls and ensure to them an unthwarted physical development by preventing them from undertaking work for which they are as yet not fit. Dr. Crampton's standard, involving, as it does, exposure of the person, is impossible of easy application. Dr. Rotch's test of the wrist bones by the Roentgen ray holds out a promise which I sincerely hope other investigators may confirm as a reliable index of the physical and mental capacity of the growing boy and girl.

Dr. Samuel S. Adams.—The main reason for my having accepted the invitation of Dr. Rotch to discuss his paper was that after having sat in the Secretary's chair for the past eighteen or nineteen years, on many occasions when the other members have betaken themselves to the recreations of the smoking room, or elsewhere, many ideas have filtered through my brain and been crystallized, and the principal one of these, perhaps, is that this Society has never taken the stand it should have taken in the matter of national affairs. Having been somewhat intimately

connected with efforts at legislation of the constructive and obstructive kinds during the past thirty years in the Capital City, my observation has been that the medical profession is more apt to be engaged in obstructive rather than in constructive legislation, and this attitude has been assumed on several occasions by this Society. If there is a body of men that is competent to speak on questions relating to the child, from his birth to maturity, this body, both as a body and as individuals, is the one. Therefore, this body as a whole should speak with authority. Now we have all seen the sad picture of medical associations sitting idly by and permitting laymen to have laws constructed for the betterment of the child and then when the laws have been passed criticising them as being inefficient and harmful, and we should now, even at this late day, after twenty-one years of existence, and having reached our majority, assume the position to which we are entitled, that of a body competent to speak with authority on all questions of public interest in regard to the health and development of the child.

The athletics of early life is the portion assigned to me in this discussion. I have been intimately connected with athletic matters in the Georgetown University and have found that my advice has been more sought after in the way of getting favors shown to these young athletes with regard to the faculty than it has been from a medical standpoint as to what boys were competent to engage in athletics.

Recently I was invited to talk at one of the principal high schools in Washington upon the effect of cigarette smoking on boys, and I was struck with the lack of thoughtful literature to be found on this subject; in fact, I came to the conclusion that those who had written on the subject were all cigarette smokers. Now I do not smoke; I tried, but it got the better of me early in life and I never tried again. You cannot stop cigarette smoking by books. You cannot convince a boy that there is enough nicotine in a cigarette to kill three men when he goes home and finds his father smoking a half dozen cigars a day. That is not the way to reach him. The effect of cigarette smoke upon the child as regards his physical development is bad. I have asked a number of genito-urinary men who have kept accurate records, and they tell me that almost invariably the boy under sixteen who comes to them is a cigarette smoker. I took the hint and investigated further as to beer drinking, and found that the boys that smoke cigarettes are the boys who drink beer; the two go together. Can we do anything in this matter? I want to throw out the hint that at some future meeting we take up this question of whether the use of the cigarette is harmful in early life and see if we may not put our stamp of disapproval upon it.

I was connected for a time with one of the leading schools in our neighborhood, where, although the medical officers were supposed to have general supervision over the boys, I was not

consulted in any instance as to their development or their fitness to engage in their work or sports, they being sent to an ignorant trainer to determine their competency to engage in athletics. The boy is, of course, anxious to become an athlete. He contests with his companion in an endeavor to decide whether he can successfully cope with them in a certain line, but does not consult the medical adviser as to his physical qualifications. I have in mind a boy on one of the teams whose physical development is so slight that his companions have dubbed him "Mademoiselle." I think we should take a decided stand in regulating the time at which the child should enter the athletic field in the primary schools, in the graded schools and in the colleges. We should have supervision over the boy up to the sixteenth or eighteenth year. Of course, after that it is difficult to control him; those of us who have sons know that.

If this principle brought out by Dr. Rotch today proves to be scientifically accurate, let us use that as a guide in giving advice on this subject. Those of us who have to deal with the schools in this respect can examine the child by this method and advise the parent whether boys should engage in certain sports or not; whether girls should engage in basket-ball or tennis. In my experience girls often endeavor to become experts in such games as basket-ball when physically entirely disqualified to stand such an ordeal. I have in mind at present one young lady who was an expert in this game and achieved a victory over a rival school, but who has been flat on her back for two or three weeks as the result of the physical strain. My advice was not asked in the matter; that was left to the physical trainer, who happened not to be a medical man. On questions of this sort, where the public is concerned, where the child's health is concerned, whether it be national or local legislation that is needed the medical man, and particularly in these cases the pediatrist, is the proper one to step forward. We have all been too modest. I some time ago urged in an address before a graduating class that every doctor engage in the politics of his locality; he should make it his business to know what laws are being passed. He need not hold office, of course, in order to do this. In my city we have no vote, our voting is done for us; we are taxed, but not represented; but you people in the states have the opportunity to exert an influence in these matters. Sometimes a word with an influential man in Congress about these conditions has a good effect. For the past thirty years I have been engaged more in blocking undesirable legislation than in bringing about legislation. Last spring a bill was introduced allowing every man who had served in the Spanish-American War, whether he had served in the medical corps of the army, or navy, or public health and marine hospital service, and who had been honorably discharged or had resigned, to register in the district without examination. That bill passed the Senate Committee of Congress, and would have passed

the Senate had we not gotten hold of it. I got a prominent senator and took him to the chairman of the committee, who stated that the bill must be all right, as no one had opposed it. I said, Why, this is a most outrageous thing! Men were put in the army who were not at all fitted as medical men, and now they can come to Washington and register and then by means of reciprocity go and practise in the states. After this explanation the bill was pigeon-holed. Many things of this sort are coming up constantly in the national legislature, and we have to be always on the alert to obstruct them. Now if you would give us a little constructive legislation it would help. In the matter of tuberculosis we sat idly by. We talked about the matter but did nothing. Then laymen stepped to the front and succeeded in getting their bill through while doctors did nothing, although the only ones qualified to give advice as to the construction of such laws. In this way we lose caste in the community. This Society should not be so modest. If our labor amounts to anything let us give the results to the public.

DR. ISAAC A. ABT.—Dr. Rotch has offered a very valuable contribution and no one can doubt that it presents possibilities, the development of which will revolutionize our knowledge concerning the child's developmental status.

The discussion of the kindergarten topics which has been assigned to me scarcely needs to be referred to at great length at this time. The paper was read at last year's meeting and has been published recently. The important facts are still fresh in our minds. Very briefly the findings are, kindergartens vary considerably in efficiency. The results obtained from kindergarten instruction depend upon the ability of the teacher, the character of the kindergarten and the child himself. The plan of Dr. Rotch of estimating the progress of development would be of great importance in selecting children who are fitted for kindergarten instruction and rejecting those who are developmentally immature. Unfortunately we have no method of estimating the physical status of the kindergarten child, just as we have no method of estimating the status of older school children.

If the child fails to keep up the pace set by the teacher, if he breaks down because he is physically unable to keep up with the tasks, then we conclude that he is developmentally below normal.

It is obvious that the information obtained in this way is a sad commentary on our lack of knowledge concerning normal growth and development and general bodily resistance.

Would it be possible to corroborate Dr. Rotch's findings by studying the same cases which he submits to his plan of investigation by making fatigue curves?

The discussion has brought out several points with reference to the position that we should take concerning the practical ap-

plication of the anatomic index of Dr. Rotch toward the solution of sociologic problems and the advisability of securing recognition from legislative bodies looking toward the adoption of this anatomic index as a standard of growth and development. Personally, I should be opposed under all circumstances to rush headlong into the political arena. I think we are not ready at this time to offer this ingenious work for universal application. I doubt if Dr. Rotch himself will sanction it. I think that the essayist brings this paper to us and desires to stimulate others to carry on the work more extensively so that it may meet corroboration, that the details may be more finely drawn and the whole subject studied in all its possible relations. It seems to me that it is our mission to stimulate and encourage scientific work to the very highest degree. It should be the farthest from our purpose to become entangled in political or legislative questions. Another point occurs to me with reference to the selection of topics for study, and that is, it seems to me, that it is more fitting that this Society should concern itself most with the problems which relate to the first periods of life. When we deal with children who are employed in factories and stores, or if we concern ourselves with child labor laws, we are encroaching on the borderland. The internist on the one hand and the sociologist on the other are already laying claim to the problems of this period of life.

Few others but we are interested in the manifold problems of infantile disease. So much work remains to be done along this line that I would think it a mistake to trespass other fields. At this moment when the whole world is interested in the subject of infant welfare and preventive medicine as it relates to the preservation of child life, we can hardly afford to forsake these topics to branch out into the wider domain.

In closing I desire to express my appreciation to Dr. Rotch for his valuable contribution. It is an original work which leads us into a new field of thought, and it should stimulate many of us to carry on this investigation. It presents possibilities of untold value along the line of classifying children on an absolute basis of physical development.

DR. CHARLES GILMORE KERLEY.—I discussed this subject at our last meeting in my presidential address, and I am glad to see that the members are so interested in the subject. Concerning the grading of schools I stated at that time that there should be a department of hygiene organized to coöperate with Boards of Education. Such a board should be made up of a skilled oculist, an orthopedist, a laryngologist, a pediatrist and a sanitarian, who should be given power to arrange suitable type and printing for the different ages, to consider problems of ventilation and lighting, of posture, of vision, of the nose and throat, of nutrition and growth and of general hygiene and to direct the recreation periods.

In considering a subject like this, a very broad one, we should

begin at the bottom and that requires inspection of the home, as to how the parents live and how their children are brought up. The first five years of the child's life are the most important ones as relates to the development. The teaching period is the most important one as relates to his future career as a citizen.

Dr. Rotch, in his paper, offers something definite. He gives us some definite index to judge by in relation to the work of children in factories and the different occupations.

Statistics, as relating to height and weight as having a bearing upon the child's capacity for work, are oftentimes misleading. This fact has been brought out recently very forcibly by the work of Crampton, who disproves the theory that height and weight should be used as an index to the child's capacity for work.

As regards the position of this Society, as to whether it shall go into this field relating to child's study in a general way, there is just this about it, if we don't do it we will simply have to sit by and see others do it because the teachers and educators generally are becoming very much alive to this subject. Recently the Board of Governors of the Teachers College of Columbia invited me to give a course of lectures to teachers on infant and child feeding —simply to the teachers, which was an innovation. The people are awakening to the idea that the medical men know something on this subject that the people as a whole should know, and if we do not take the initiative in the matter some one else will.

DR. L. EMMETT HOLT.—I should feel sorry to see a large part of the work of this Society devoted to subjects of this kind which, though of sociologic interest, are not so much along the line of work of most of us as other matters more strictly medical. I believe we can do our best work along lines of research. No other society in this country stands for that as does this one. Insomuch as the work is research work it should be encouraged, and I should certainly place Dr. Rotch's work in this class, but I am not so sure that it is the function of this Society to go into the sociologic part of it. Dr. Abt's ideas are very much those which I entertain. We should be very sure of our position before attempting to put the matter before the public. This anatomic index should be corroborated by the observations of a number of workers before we, as a society, go on record as endorsing it unreservedly. It looks most encouraging, but we know many observations are necessary to get at the whole truth, and these must be made under many different conditions.

We have a duty toward the public, but that should not be the most important side of our work and we should be careful not to take too positive a stand upon this question until sufficiently sure of our facts.

DR. MORSE.—Dr. Holt and Dr. Abt have expressed my feelings on this subject very much better than I could express them myself. I agree with them fully.

DR. FREEMAN.—This work of Dr. Rotch's is very important so far as it has gone, and there has been sufficient of it for us to accept it without further corroboration. But he has not shown the relation of the development of the carpal bones to the age of the child, the general development of the body, dentition, growth of pubic hair, and so forth, all of which, it seems to me, is very important in order that we may judge of the value of carpal development as an indication of general development.

DR. CARR.—In reference to the study of sociologic questions we are in a position analogous to the churches. They were organized for spiritual work, but go beyond that to reach the people. We were organized to study the diseases of childhood, and the question is, How far can we go into outside matters? Personally, I think many of us as individuals, as members of committees, or through some affiliations we may have, can do a great deal, but I believe that the work of this Society should be largely medical.

DR. ROTCH.—In regard to Dr. Freeman's remarks, I have written on this subject before, and in my paper I stated that this is only one of the factors of the problem and that we must work out all the factors one by one. Because, however, it is only one I see no reason why we should not take it up. All the evidence is in favor of its being correct at present, as is believed by those who have investigated it.

As to devoting ourselves to medical subjects strictly, it seems to me that all over the world it is recognized that prophylaxis is of the greatest importance. To say this is not medical, and that work of this kind should be left to the laity is, I think, absolutely wrong. I believe it is our duty to the public to give them the benefit of our knowledge, and what I have said today is just as much medical as any scientific paper that can be read. What is the use of making these investigations if we are not going to make use of their results for the public good? I see no reason why these are not medical subjects, and I believe they should be taken up by this Society. I do not think the matter should be put aside because we happen to have said a little more about it today than about anything else. The facts presented are medical facts. In New York great numbers of children have been taken out of the sweatshops and mills, but having been taken out the question arose as to what to do with them. According to their age they were put in institutions regardless of fitness. The physician should be able to answer these questions regarding the fitness of the child. We are not entering into politics in this any more than in anything else. How are the lawyers and judges, who have spoken to me over and over again upon this subject, to decide what is best for these children if we as pediatrists cannot tell them? Some one must say what is to be done with the child, and who are more competent to do so than the members of this Society? If we take up the matter of height and weight, as Dr.

Crampton has done, we find that the statistics are illusive and misleading, and we cannot make up good laws from these statistics. If there is a better method of determining these questions we should use it. If we take an individual child and see whether it is ready to do a particular work, that has nothing to do with statistics. The matter of height and weight is not sufficient, and if we go on using such methods we are behind the times.

We have nothing to do with legislation, but if we are afraid to stand up and state positive facts it is not a step in the right direction. Prophylaxis is the great principle of the medical work of the future. I brought this up merely to show how to make a beginning, because something should be done. We must treat the child as an individual and not according to general statistics. Of course everything needs corroboration. We have no idea of making this the whole work of the Society. The Roentgengraph is giving us a means of investigation now which we did not have before. I supposed that of course this Society as a whole would wish to lend its influence to broad questions of this kind connected with pediatrics. If, however, it does not so wish, these questions can be discussed elsewhere.

A FEW OF THE THINGS A PEDIATRICIAN SHOULD TEACH.

BY P. J. EATON, M.D.,
Pittsburg.

In developing the subject I have chosen, I shall not try to tell you anything which you do not already know, nor shall I attempt to bring forth any new or startling facts. Yet some of the things I may say are so simple that possibly they have been passed over in your teaching.

I have been so deeply impressed by the utter lack of real knowledge in regard to infant life as seen in the work of many general practitioners, and in the conduct of families by the parents and nurses, that I have, in my own practice, in my lectures to nurses, and in my teachings, tried to bring home to those who should be interested some common-sense facts, and some principles which one could almost dignify as axioms.

When we consider that nearly all children come into the world strong and healthy, when we remember what a large proportion die before their third year, and when each of us recognizes the fact that lack of knowledge on the part of those who should know is responsible for much of this great loss of life, the necessity for the simple common-sense teaching of parents and nurses, and of the growing generation of young women too, becomes apparent.

Very few people, be they fathers, mothers, nurses or doctors, think of their responsibility to the child, and through it, to the state; or, if they do realize their accountability, many deliberately neglect their opportunities for the betterment of the race.

Strong, healthy men and women, with alert minds in physically competent bodies, both governed by a steadfast moral character, are what is wanted by this nation, else decadence will begin. Our sociologic instincts have of late years been stimulated, and much is being done to ameliorate the deplorable conditions which surround the average citizen, but I most emphatically declare that the physical, mental, and, of necessity, moral training, should begin with the birth of the child, and I further believe that no

body of men are so competent to teach the true principles of right living, as those who love and are interested in little children.

Now, how shall we, who recognize our responsibility to the young of our race, best teach and practise that we may obtain better results; results which shall leave an impress for the general betterment; results which shall be object lessons for those who see, and, seeing, be led to imitate?

Among other things, we should teach that babies have certain rights—the right to live, the right to be comfortable, the right to grow to manhood and womanhood in the image of God and not of the devil.

I believe that to live, to be comfortable, to grow, are dependent on the right following of fundamental rules which should be in force, and enforced from birth. First, we must teach regularity, the cultivation of accurate habits in the baby; make a machine of the little one. Teach it to employ its various functions at fixed and convenient times.

Should we teach that a child should be fed at precise intervals, and that it can be trained to digest? It is a fact and not a theory that gastric juice is not present all the time in the stomach in quantities sufficient, and it is also a fact that when a stomach is led to expect food at given intervals there *will* be gastric juice there, or ready to be put there: whereas, when food is ingested when the stomach is not so supplied, the food must remain in the organ—warm, moist, already with ferments in it, being rolled about and generating unwanted and unwonted chemical products; *or* the stomach is called upon to manufacture gastric juice in a hurry, and perhaps when, only a comparatively short time before, it had done its true work.

Has any one the right to ask the delicate economy to undergo any of these conditions? Why is it necessary to emphasize this? Because most mothers think that if the *intervals* between feedings are correct, the scheme of regularity is being carried out. Far from it.

Shall the child be awakened for its food? Nay, say all the grandmothers. Yea, says the scientific medical man, and for the reasons already enumerated. And yet, if the scheme of absolute regularity be carried out, such a "creature of habit" does the stomach become that the child will, as a rule, at the prompting of its inward monitor, wake for its feeding at the right time. Ex-

perience teaches that, in prescribing the interval of feeding in any given case, the actual hours for feeding must be *written out*, and the mother be impressed with the fact that ten o'clock means ten—not 9:30 or 10:15 and so on. Plainly put, these facts can be readily understood by the average parent or nurse.

It is very easy to teach a very young baby what is expected of it in the way of regular defecation and often the feel of the "potty" underneath its buttocks, and the upright position, will give it all the hint necessary, and the results follow—an economy in washing and a mighty good habit. The bath should come at a fixed hour, too, followed by a meal and a nap. Simple points all, but worth teaching nurses and mothers.

Can children be taught to sleep? Most assuredly. When the sequence of the daily routine has brought around the sleep hour, *prepare* the child today as yesterday—put it in its bed, darken the room and leave it *alone*. We used to be told that "the hand that rocks the cradle rules the world." The modern and correct version is that "the hand that rocks the cradle spoils the world." Rocking, patting, singing, all are pernicious habits. The trained habit of sleep becomes soon almost automatic and the usual house sounds are unnoticed.

I said that the child has a right to live, but living means proper food as well as proper feeding intervals and proper sleep. We can all assume and teach as an axiom that the Lord knew best when He made breast milk for babies, and it as truly follows that if we do not in individual cases have breast milk in sufficient quality and quantity we must give that which most nearly approaches breast milk physically and chemically.

Man is ingenious, but with all his ingenuity he has not yet made an artificial food which can equal normal breast milk as a food for growing infants. The wisdom of Solomon is no longer abroad in the land, and he who says any one patent food is the *only* substitute or is a *genuine* substitute for breast milk, is nearer the class of the Ananiases than that of the Solomons. No one yet has devised a food nearer, physically and chemically, to breast milk than is modified cow's milk. Raw, sterile, with its relations of fat, proteid and sugar correct for the given infant, it will do next best to normal breast milk. Teach the mothers and nurses what good milk is and where it can be obtained.

Direct your mothers to fasten securely the pages of their

nursery guide which are devoted to the feeding of babies. Those alluring tables are the result of the good work of an eminent man over many years, and are, as it were, a composite photograph of all the food formulæ as applied to babies *en bloc*, but you and I do not have *average* or composite types of babies brought to us. Each one is an entity, different in one or many characteristics from each other child. I beg you to teach that it must be fed as a unit, giving it that which suits its individual needs, regardless of weight, age or other conditions as put forth in the feeding tables. To do it is to succeed in a very large number of cases. Teach the mothers that the living skillful medical man is superior to the printed book.

In its pursuit of happiness and comfort the child should be suitably clothed, and I submit that while our modern way of dressing babies has much in it that is artistic and lovely so far as *looks* go, it is conceived on a wrong basis. Did you ever observe that the baby is turned or rolled over from six to twelve times while its clothes are being put on or taken off, that it is held up by the heels while its trousers are shaped upon it, and that it is pried up by the back of the neck to finally button its slip? Why should its clothes not all button up the back or all button up the front? The less the baby is handled the better for it. To open the minds of the young, growing generation of girls to this fact means, in time, sensible clothes for infants.

We clothe babies that we may conserve the body heat and make less deadly in their effects the great changes of temperature which obtain in our inhospitable climate. We want next the baby's skin a protecting layer, if you will. I believe this end can best be obtained by clothing the child, as to its underclothes, in linen mesh, which in these days can be obtained in sizes becoming to all ages. Then, having this layer of constant protection next the skin, put on outside garments according to the thermometer and the wind. Most people dress their children as the street railway heats its cars—*i.e.*, by the calendar and not by the thermometer. I have seen children brought to my office in December when the temperature outside was 50° F., clothed as for an arctic expedition, and sweating like a running Malay. What an elegant set of conditions for the inception of a bronchopneumonia!

Naturally enough comes next the consideration of the amount

of fresh air the child should have, and its relation to the health of the child.

The growing child needs lots of oxygen for its proper tissue metabolism, and it can best obtain it if kept in the open air most of the time. It does not harm babies to be in cold air if properly protected, but to be shut up secure from fresh air causes much deterioration in their condition. The child, after its first few days, should be in the open air daily (weather permitting) for as long as can be arranged for, and it should always sleep at night in a room in which is free, flowing, out-of-doors air in abundance. An earnest word now and again will aid in convincing people that most of the illnesses, even pneumonia, can best be treated in the open air.

As the child grows, the question of diet becomes a very live one, and many a child is ruined because of the anxiety of parents, or more often of grandparents and other relatives, that it should eat solid food. An exceedingly good rule to follow is, "Things which need chewing must have used upon them teeth capable of grinding." And, because digestion begins in the mouth, almost all solids must needs be masticated thoroughly that the necessary physiologic processes may go on in the proper way. As the child grows add to the diet those things which common sense will indicate—simple, easily-digested articles--junket, custards, well cooked cereals, broths, meat juices, stewed or raw fruit (put through a sieve) and so on.

The question concerning the effect of dentition upon the general and special conditions of the child is a live one, and what little teaching there is is apt to be erroneous.

Some there be who consider that it is a crime to put lancet to the gums, no matter how much pain the child may be suffering, or how much upset it may be.

Teething is a physiologic process, they argue, and should be let alone; and further it is held that there are no symptoms directly referable to dentition. Let us teach that teething is certainly a physiologic process when *normal;* and, if normal, no interference is necessary; but when the gums are very tough, when the teeth are forced along rapidly, the discomfort and pain are great. It is easy to prove that the irritation of the mucous membrane by the pressing teeth causes a change in the reaction of the saliva, and prevents normal digestion. A piece of test

paper will often show an acid reaction of the saliva instead of the normal alkaline reaction, and one often sees a complete relief of gastric symptoms after the offending tooth has been freed. Imagine, if you please, the effect on the delicate nervous system of a growing child of the simultaneous pressure of four rapidly pushing anterior molars. I have pried open the mouth of a child in convulsions, cut the inflamed gums over several teeth, and with no other treatment have seen the spasms cease—not to return again. As a sweetener of the child's disposition, there is, at times, no remedy the equal of the gum lancet—anorexia, peevishness, temperature, sometimes accompany the painful attempt to erupt teeth, due, I admit, only *indirectly* to the teeth as anatomic entities, but, nevertheless, symptoms to which attention must be paid. All of you who have studied children to any extent have undoubtedly seen cases where the only sign of dentition was a coryza. Further examination will often show an irritated mucous membrane over certain teeth, and the relief of this irritation by freeing the teeth is always followed by the disappearance of the so-called "cold."

Defecation is a physiologic process, yet who would contend that laxatives or cathartics or enemas were never to be advised? That one should wait till nature or the physiologic processes had done their work?

The man who would so argue would be lacking in common reasoning power. Common sense, the hard thinking, dispassionate kind, which reaches reasonable conclusions as to the matter in hand, is what counts most in the treatment of little ones.

Some years ago I had occasion to make some remarks at the Academy of Medicine on the conditions met with in the prepuce of boy babies, and something as to the care of this part of the body. In looking up the literature, I was amazed to find how little attention had been paid to the subject by most authors, four lines by Dr. Rotch being the maximum of discussion. I believe that we should teach, as my old professor of surgery taught, that the toilet of that part of a boy's body was as important as that of his eyes, ears, or nose, and that if the natural or customary adhesions between glans and prepuce do not disappear in the early weeks, the preputial orifice should be dilated if necessary, the adhesions broken down, and a simple dressing with sterile vaselin applied. Then the attendant should be instructed

to thoroughly cleanse the area in question daily and apply the ointment till the membrane has thoroughly healed. And that thereafter the parts should be cleansed daily. I believe that there is less likelihood of the formation of bad habits later if the mucous membranes are accustomed to ordinary stimuli. I also teach that the corresponding parts in the female infant shall have equally good care, for I have seen many instances where later in life the preputial irritation in girls has been a source of much trouble and inconvenience. These are a few of the things that we, who love children, should teach, and many are the opportunities which fall to us in our practical work among our colleagues, as well as among the parents and nurses.

In the June, 1906, number of the *Outing Magazine* is an article by Copley, which I commend to the attention of each one of you. Its title is, "Give the Baby a Chance," and while it gave me much personal encouragement and comfort, it has served to enlighten the minds of many mothers and grandmothers to whose attention it has been called, and to ameliorate the life conditions of many children who were suffering from too much, or too little, so-called *care*.

DISCUSSION.

DR. MORSE.—I have expressed my opinions on the subject of interference with the genital organs of children so many times that I am afraid that if I do not do it again it will be assumed that I have changed my mind. I object to circumcision, and even to the retraction of the foreskin, now as I have always. It is a normal condition for the prepuce to be adherent in small babies and to remain adherent for a good many years. These adhesions very seldom cause any disturbance. When they do it is time to interfere, not before. The habit of masturbation is, I believe, not confined to Christians. It seems to me that this interference with the genital organs of small children is, to a large extent, the manifestation of a morbid interest of one sex in the genital organs of the other. It is, in my opinion, far more often suggested by nurses than by physicians. In many cases it has seemed to me that the only reason for interference was the chance of an operation with the consequent fee. Interference is, in my opinion, in most cases unjustifiable.

DR. EATON.—In reply to Dr. Morse, I would say that while I thoroughly agree with him in his contention that the prepuce is ordinarily adherent, I do believe that many cases of disturbances

are caused by a large collection of smegma beneath the prepuce, and that such cases ought to be operated on at once. A circumcision is almost never required, but simply the breaking up of the adhesions and the cleansing of the glans is sufficient. It has not been my experience that it has been suggested by nurses, but has been called to my attention by the behavior of the babies in a large number of cases.

This week I performed this simple operation on a baby and removed from beneath a distended prepuce a very large amount of smegma. This was causing much pressure, and the child's attention was constantly attracted to its genitals. I do not at all wish to be understood as advocating interference in every case, but do believe many cases need this attention which do not receive it.

SPASMODIC STRICTURE OF THE ESOPHAGUS—A CASE DEMONSTRATED BY THE X-RAYS.

BY L. E. LA FÉTRA, M.D.,
New York.

To the pediatrist, vomiting in young children is always a very annoying and frequently a formidable symptom. As our knowledge of digestion and of developmental abnormalities increases, we find that there are being added to our list new types and additional causes of vomiting. Of recent years our attention has been drawn to the frequency of the vomiting due to stenosis of the pylorus. Last year Dr. S. S. Adams presented before this Society the report of a case, with autopsy, of spasmodic stricture of the esophagus. In his case there was a circular narrowing of the esophagus five inches above the cardiac opening of the stomach. There was found also an enlarged bronchial lymph node adjacent to the stenosed portion of the tube, and above the narrowed esophagus there was a diverticulum. Cases of cicatricial stricture of the esophagus following the ingestion of corrosive liquids are not very uncommon and are very readily recognized. The case to be reported is interesting principally from the standpoint of its diagnosis.

The child, Baby M., aged sixteen months, was seen in consultation with Dr. Everett M. Hawks on April 25, 1909, on account of attacks of vomiting. The baby was born at full term and was apparently normal. On the third day after birth there was discharge of dark blood from the mouth but no subsequent bleeding. Nursing was attempted for three and one-half weeks, and then the mother's milk failed. The baby did not thrive on the breast milk. There was vomiting or regurgitation at or after every feeding. The stools were small and apt to be constipated. Bottle feedings were then given, first of modified milk, then of oatmeal gruel with milk, barley gruel and milk, Mellin's Food and milk. The baby always did fairly well on a new food for about one week and then began to regurgitate. The vomiting was always without fever, except in three attacks which were more serious and were accompanied by a temperature of 102° to 103° F. Careful questioning brought out the fact that the vomiting usually occurred while food was being taken, seldom afterward. When it occurred one and one-half to two hours following the

feeding, there were always strings of mucus ejected. At times the baby would take and keep down as much as eleven ounces, and for one period of ten days as much as fifty ounces of food had been taken and retained each twenty-four hours. When the baby was one year old she weighed fifteen pounds. During February of this year there was a severe attack of vomiting and the baby lost weight down to ten pounds. Under the care of another physician who was called into the case the vomiting lessened and the baby began to gain so that by March 23 the weight was thirteen pounds. The quantity of food was increased up to twelve ounces at a feeding. Then the baby began to vomit badly and became constipated. It was at this time that the child came under the care of Dr. Hawks, the physician with whom I saw the case and to whose courtesy I am able to report the case. At his first examination the baby seemed normal save for emaciation and constipation. There was no vomiting. The baby was hungry and took a large amount of food. The stomach was washed out four times. Each time the tube passed down about six and one-half inches and there met an obstruction. It could be forced down about another inch beyond this. A large quantity of mucus was obtained, and the baby would vomit alongside the tube. Moreover, it was found that at the first feeding in the morning, after the stomach had rested all night, the baby would vomit after taking about four spoonfuls of food, bringing up as much as a teaspoonful of firm yellowish mucus together with the fresh food. Then the baby would swallow the food well and there would be no vomiting. If the stomach tube were passed about two hours after the first feeding, mucus would be obtained, and this was probably not from the stomach.

The baby was first seen by me on April 25. At that time she was emaciated and her weight was twelve pounds, eight ounces. She had a loose, rattling cough, but the chest was apparently normal. The lungs and heart were negative, as was the throat. There was no distention of the stomach; no peristaltic waves and no pyloric tumor; the liver was two fingers' breadth below the ribs and the spleen slightly enlarged. There was no dullness alongside the larynx and trachea, the examination being made before the feeding. I watched the baby being fed from a spoon. After a few mouthfuls the baby regurgitated some mucus with the food. Then she took about two ounces, stopped and began to fret and put her left hand down her throat and brought up as it

were a handful of mucus and food. Then she went on and took the balance of the feeding, some six ounces, without fretting. If her attention were diverted she would keep the food down, and there was usually no vomiting until the next time of taking food.

It was thought from the time of the vomiting and the way in which the child vomited that there must be a pouch of some kind alongside the esophagus. The absence of peristaltic waves on the abdomen ruled out pyloric stenosis. It seemed reasonable to believe that the mucus vomited first came rather from the pouch than from the stomach, and accordingly it was ordered to pass the stomach tube and use an alkaline saline solution for irrigation every second day before the evening feeding. The baby was put upon a malt soup mixture.

On May 7 the baby was seen a second time and the condition found about the same. It was noticed that the stomach tube met an obstruction before entering the stomach and there was usually peculiar noisy breathing and cough immediately preceding the vomiting at the time of the feeding. The baby did not cough between feedings because the cough was always brought on by swallowing. There was a continuance of the habit of putting the hand down the throat and bringing up food and mucus, the mucus being yellow and mixed with very little food. Percussion of the front of the chest was made before feeding and no dullness found. After the feeding distinct dullness was made out to the right of the sternum extending from the first to the fourth spaces. On account of the statement of the mother that the baby vomited much worse when she was excited, one-twenty-fourth of a grain of codein was ordered to be given immediately after two of the feedings. The malt soup was continued. As a result of my second examination I felt quite sure that a pouch would be found to the right of the esophagus, and on May 8th radiograms were made by Dr. E. W. Caldwell, with the assistance of Dr. Hawks, both before and after feeding. Two drams of bismuth subcarbonate were used in six ounces of food. On May 11th the baby had made a gain of one pound from May 7th. The vomiting had been much less since the administration of the codein and the baby was able to take very thick liquid food. As a result of the X-ray plates it was decided to give the codein before two of the feedings and before the other to use 15 m. of olive oil. It was thought better to use no irrigations of the distended esophagus.

The X-ray picture taken before feeding showed nothing abnormal. Incidentally the centers of ossification of the sternum are shown very clearly. The X-ray plates taken after the feeding which was mixed with bismuth showed a dark shadow extending down to about the seventh dorsal vertebra. This shadow ter-

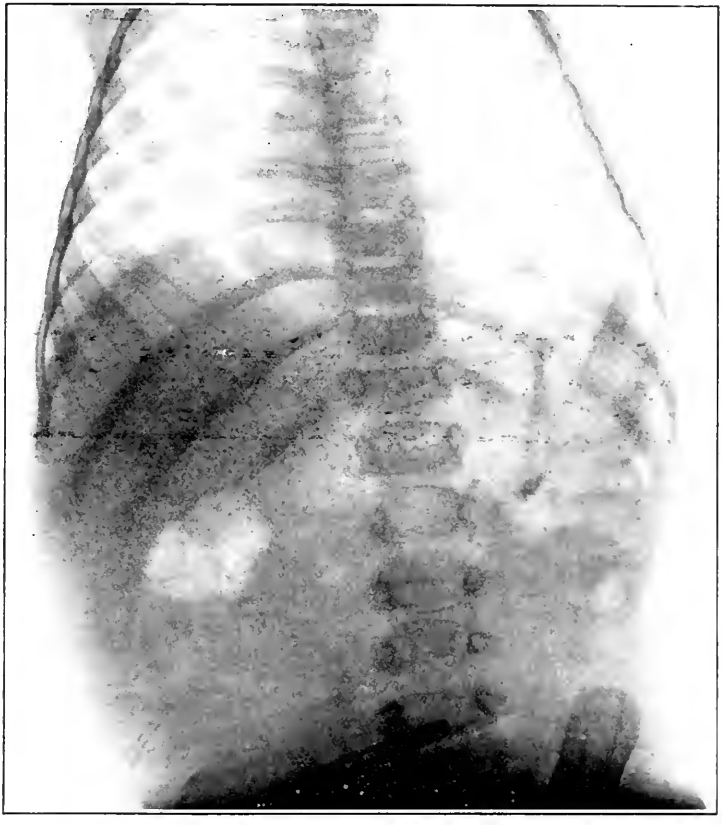


Fig. 1. Antero-posterior view taken before feeding. Note that the area to right of the sternum is clear—a normal picture.
X-rays by Dr. E. W. Caldwell.

minated in a conical shaped pouch directed toward the left. Below and to the left is seen the shadow which represents food mixed with bismuth in the stomach. The stomach is apparently contracted, giving somewhat an hour-glass appearance. Between the stomach and the lower end of the esophageal pouch there is no intensification of the shadow. This would seem to mean that all the food with the bismuth had been passed entirely through

that portion of the esophagus and would seem to contraindicate an organic stricture. If there were an organic stricture it is reasonable to believe that some of the food would have been caught and held along the tract.

The radiogram taken from the side showed that the distended

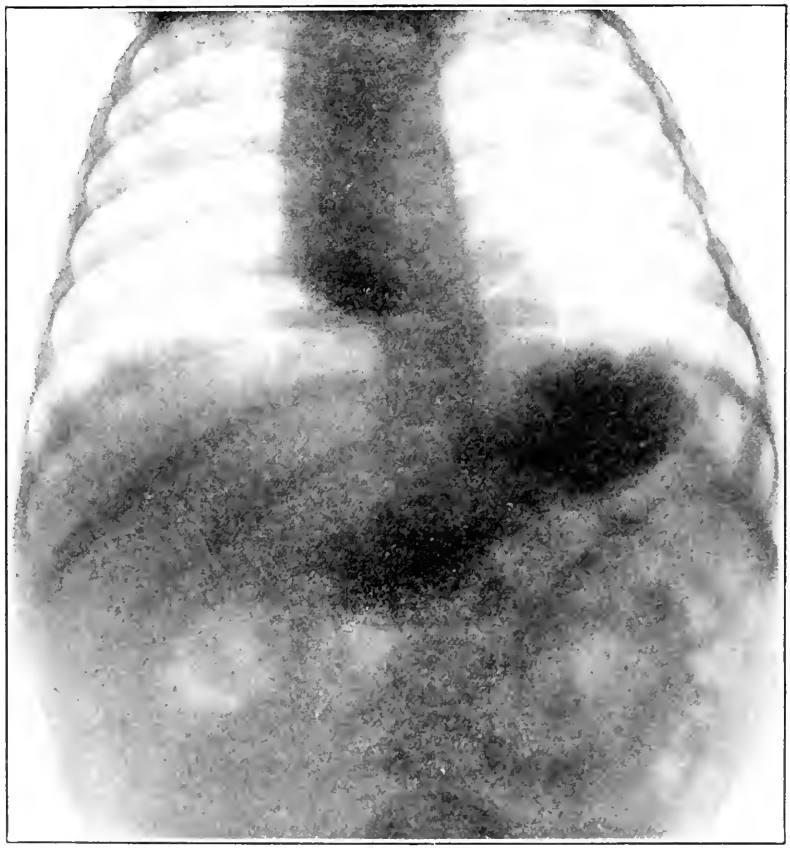


Fig. 2. Antero-posterior view taken immediately after feeding, in which were mixed 2 drams of bismuth subcarbonate. Note the dark shadow at the right of the sternum, most intense at the lower part, where it is conical and directed toward the left. This shadow represents the esophagus distended above the stenosis at the cartilage between the seventh and eighth dorsal vertebrae. Note also the deep shadow showing the stomach in peristalsis and the absence of any dark line or shadow connecting the stomach and esophagus.

esophagus occupies its normal relation to the spinal column antero-posteriorly. In this picture there is also a clear space between the lower portion of the distended esophagus and the stomach.

I wish here to acknowledge my indebtedness to Dr. Caldwell

for his careful radiograms, as also for his assistance in the interpretation of the plates.

As regards the classification of this case, it is one of spasm

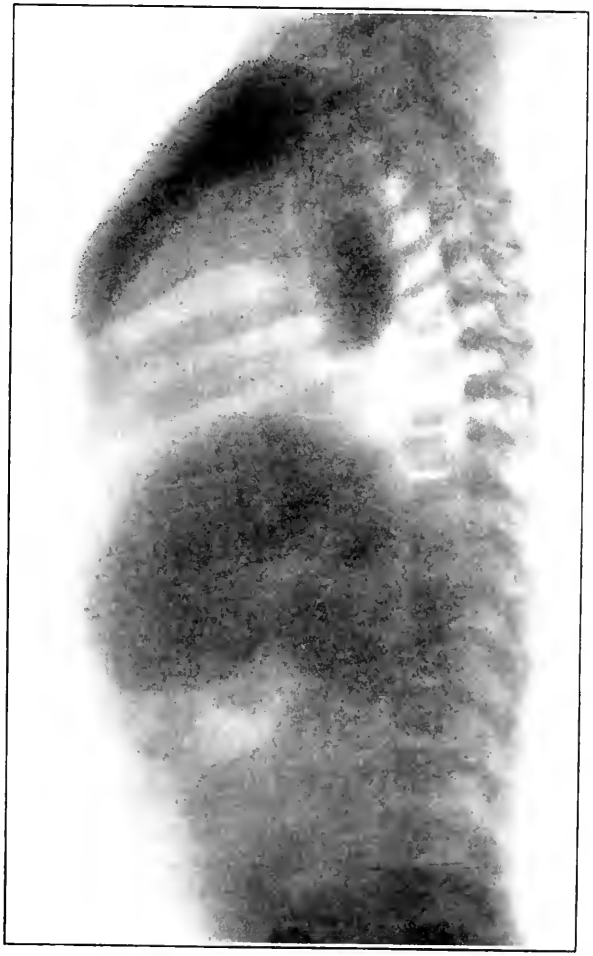


Fig. 3. Lateral view, taken after feeding. Note the distended esophagus in its normal position in front of the vertebrae. The stomach shadow is merged with that of the liver.

of the esophagus itself rather than cardiospasm; for the stenosis in this instance is at least two inches above the cardiac end of the stomach. There is found, however, the same distention as occurs in cardiospasm only at a higher level. This distention

produces a pouch which acts as a forestomach or "Vormagen." This serves as the reservoir for food and empties by jets into the true stomach.

The passage of metal sounds or of firm bougies was not attempted in this case because of the disastrous results that have been reported from such procedures, not only in the literature but especially at the discussion of Dr. Adams's case in this Society last year.*

Dr. Adams said that but 1 case resembling his could be found in the literature at the time of his report. I have gone through the literature appearing since that time and do not find another. There was, however, one article, a paper on cardiospasm by Guisez and Léopold Levy which I was unable to consult, since the volume containing it was at the binder's. During the past year there have been numerous reports of cicatricial stricture of the esophagus in children, the most notable being that of Ashurst; and there have also been a few reports of spasmodic stenosis or spasm of the esophagus. All of the latter, however, have been cases occurring in adults or older children and have been attributed to hysteria or other marked nervous disturbances.

DISCUSSION.

Dr. Rotch.—This is an exceedingly valuable presentation. It shows what the Roentgen ray can do, and it is certainly a case where the Roentgen ray is needed in examination.

I have had one case occurring in the lower part of the esophagus, before the time of the Roentgen ray, and we were afraid to pass the sound. The symptoms corresponded to what we get where there is a foreign body or obstruction in the esophagus. It is very difficult to differentiate between pyloric stenosis, dilated stomach and spasm of the esophagus. It is a very valuable thing to have aid of this kind. It is wonderful and true to the letter. We now have a method by which we simply turn the leaf of the book and read what is the matter. With my patient it was shown postmortem that we were right in not using a sound, as there was a perforation of the sac.

These Roentgenographs represent most beautiful presentations of our modern advanced methods of diagnosis, and Dr. La Fétra is certainly to be congratulated.

* On September 17, 1909, I learned from Dr. Hawks that the baby has done fairly well during the summer, vomiting much less (mostly mucus, and at night) and gaining in weight. The codein had to be discontinued, and tincture of belladonna was used instead, to check the secretion of mucus in the pouch as well as to relax the spasm.

A CASE OF SO-CALLED CONGENITAL MALARIA.

BY FRANK S. MEARA, M.D.,
New York.

The subject of this communication was admitted to Bellevue Hospital during the first week of December, 1906, at the age of seven weeks. The child was born in the middle of October at Massapequa, Long Island. The date has significance with reference to the mode of infection.

Family History.—The mother was a young woman in her early twenties, a Russian Pole, who came to this country three years before. She lived in New York City for two years. One year ago she married and moved to Massapequa, Long Island. Last August, when seven months pregnant, she was taken ill with chills, fever and sweats. Her chills and fever continued for three weeks. Most of this time she was confined to her bed. She was finally apparently cured by quinine. Since then she had no other manifestations of her malarial infection except boring frontal headaches and malaise.

Her husband was sick at the same time and in the same way and was confined to his bed for one week. He, too, was cured by a course of quinine.

For two weeks prior to the birth of the child the weather was freezing every night at Massapequa. The mother was absolutely positive that there were no mosquitoes about the house or out of doors since the infant's birth.

For the first two weeks of its life it was what the mother called a large, healthy baby, but *at the age of ten days or two weeks*, though on the breast, taking its food fairly well and having yellow stools, it still seemed to cry a good deal and lose weight. For another week this loss of weight continued while on the breast and the color began to be poor. The mother now became thoroughly alarmed and took the child to New York, where she carried it from clinic to clinic seeking relief. In the meantime a swelling developed in the groin and scrotum, for which latter condition she brought the child to Bellevue.

The little patient had had some cough for four weeks before admission. He had nursed at the breast for five weeks and since that time had been on cow's milk. He had been constipated and of late had vomited twice a day.

The child had had no symptoms that could have been interpreted as infection with the malarial organism; neither fever, chills, convulsions, cyanosis nor gastrointestinal disturbances other than those mentioned.

The discovery of the real condition was due merely to the findings of the routine examination, as the case had been admitted as a hydrocele of the cord.

Examination.—The patient is a male child, strikingly anemic. The head measures 13¼ inches in circumference; chest at the nipple line, 13 inches; abdomen at its most prominent point, 12¼ inches; length, 20 inches. The weight is 7 pounds, 9 ounces. The anterior fontanel measures 1¼ by 1½ inches. The posterior fontanel is closed. There is a slight craniotabes. The conjunctivæ are pale, otherwise the eyes are negative. There is a slight coryza. The mucous membranes of the mouth are pale. The cervical glands are shotty, the axillary palpable, the epitrochlears not felt. The chest is normal in outline. Auscultation reveals both in front and behind coarse rhonchi and a few fine râles when the child takes a deep breath. The heart sounds are normal. The abdomen is prominent and there is a diastasis of the recti muscles with a slight protrusion between.

The spleen fills the whole left side of the abdominal cavity, the edge reaching the middle line. The edge is fairly sharp and when the muscles are relaxed the notch can be beautifully made out.

The liver edge is 1¾ inches below the costal margin, which in so small a child reaches nearly to the level of the umbilicus. There is swelling in the right side of the scrotum and in the right inguinal canal, due to a hydrocele of the cord.

The blood was examined and found loaded with malarial organisms. The mother's blood was also examined and found to contain the organism.

Blood count gave 11,000 white cells; polymorphonuclears, 16 per cent.; lymphocytes, 84 per cent.

Only on three occasions did the temperature go above 99°F., after the first day when it was 99.5°F. On the second day after

admission it was 100.4°F., on the eighth day 100.8°F., and on the tenth 100.6°F.

It was put on a modified milk mixture, representing a caloric value of between 350 and 400.

The child lost weight rapidly, and this loss of weight was the most significant expression of its infection and certainly could not be attributed to lack of nourishment. In three days it dropped from 7 pounds and 9 ounces to 7 pounds and 3 ounces; in two days more to 6 pounds and 11½ ounces and in two more to 6 pounds.

The bimuriate of quinine and urea was begun in gr. v. doses three times a day on the 11th. The child died on the 14th.

I am indebted to the Pathological Department, under Dr. Chas. Norris, for the report appended.

"Blood films of December 11th: Blood shows marked poikilocytosis, great variation in size of red cells, numerous nucleated reds, mast cells and a few myelocytes. Plasmodia malariæ of the tertian type are found in great numbers and great variety of form. There are numerous signet and ameboid forms, as well as fairly numerous sporogenetic forms within the red cells. No extracellular forms were found in the slides examined. Films examined on the following day (December 12, 1906) contained practically the same forms of plasmodia as those described above.

"Attempts to demonstrate flagellate forms by incubation, followed by staining, were unsuccessful at this time.

"Examination of the smears made at the time of autopsy from the spleen were wholly unsuccessful in demonstrating presence of plasmodia in this organ. This failure may be ascribed to the comparatively long period of time elapsing between the death of the infant and the postmortem examination.

"On December 14th, examination of the stained blood films from the mother of the patient showed numerous signet and ameboid forms of the tertian parasite."

The autopsy showed a large spleen corresponding to the size made out before death.

Before Laveran's studies of the blood in malarial subjects, the pathogenesis of such a case as that cited above would have had, and, indeed, did have, but one interpretation, an infection of the fetus in utero by the mother; but with the knowledge of the protozoon as a causative agent and the further knowledge of

its life cycle and the mode of transmission from mosquito to man, there arose a double question as a bar to the acceptance of intrauterine infection; first, the permeability of the placental structures to such an organism, and, second, the exclusion of the ordinary method of extrauterine infection.

These doubts have steadily grown into a certainty in the minds of most students of the disease in question, and yet that there is not an entire unanimity of opinion on the part of authorities is startlingly obvious on consulting the remarks of the two men contributing articles on this subject in the last edition (1905) of the *Traité des Maladies de l'Enfance* of Grancher and Comby. In Vol. V. of this treatise J. W. Ballantyne, of Edinburgh, writing the articles on "Maladies de Fetus," says, with reference to malaria, that "several observers have shown that malaria can attack the fetus in utero," and quotes R. W. Felkin (*Transactions of the Edinburgh Obstetrical Society*, 1889, Vol. XIV., p. 71) as having noted a case in which the infection seemed to have been of paternal origin rather than maternal. This report, while justified in 1889, when Felkin wrote, goes unchallenged in 1905 by Ballantyne, who further remarks: "Personally, I have observed at the Royal Maternity of Edinburgh a case in which the woman, during the first days following her labor, had recurrent attacks of intermittent fever. Her newborn was likewise attacked, and in the blood of both were found the characteristic hematozooa of malaria. During pregnancy the fetus can have chills synchronous with those of the mother, and which can be perfectly well recognized. In these cases the child is born with a large spleen." These last sentences of the quotation, following on the statement of his case, would connote his own acceptance of intrauterine infection.

In Vol. I. of the same treatise Luigi Concetti contributes the article on "Malaria." He says that the idea that malaria can be transmitted from the nurse to the infant by the milk or from the father to the child by spermatozoa must be dismissed. Discussing maternal intrauterine infection, he says that a case of Bein is the only one in which findings in the blood of a newborn of the parasite was at all positive, and in this case the blood was not examined until several days after birth, which does not exclude the possibility of infection after birth.

Thayer, in his lectures on the malarial fevers, 1897, speak-

ing of congenital malaria quotes a case reported by Duchek as the most positive with which he is conversant, concerning a child of a malarial mother, which died shortly after birth, and showed on autopsy an enlarged pigmented spleen and pigment in the portal vein.

Laveran himself (see Peters' article) was a champion of this mode of infection and quoted to support his view in addition to the case of Bein and Duchek, others reported by Sue, Hawelka and Bouzian.

The negative results of careful investigations, with reference to this mode of infection, are more convincing than any positive reports yet made, none of which will stand rigid criticism.

Thayer (*loc. cit.*) studied the blood of a child born at the eighth month during a malarial paroxysm of the mother. In the mother's blood were three groups of the quartan parasite; the child's blood, on repeated examinations, was free from parasites or pigment. Examination of the placenta showed parasites and pigment on the maternal side and neither on the fetal side.

Among others who have studied the blood of fetuses of malarial mothers with negative results are Bignami and Guarnieri, Bastianelli, Caccini, Pennato, Ferroni quoted by Concetti, Marchiafava and Bignami quoted by Peters, and S. Serini quoted by Pezopoulos and Cardamatis. These are names, the connection of which with malarial hematology must carry weight.

The last detailed study of this group of cases was carried out by Pezopoulos and Cardamatis, of Athens. They examined the blood of 6 mothers suffering from malarial fever and the blood of their newborn. They also examined the blood of the placenta in 5 cases both from the maternal and fetal surfaces. Two of the infants dying one to two days after birth were autopsied and sections of the liver and spleen examined. Sections were also examined microscopically from the placenta.

The results of these examinations were as follows: In the blood of these 6 mothers there were malarial parasites more or less abundant.

In the blood of the infants and the aborted fetus there were no parasites.

In the blood of the liver and spleen, as in the sections of these two organs of the infants autopsied, there were no parasites.

In the blood of the 5 placentas taken from the maternal sur-

face there were malarial parasites in abundance and the most of the forms undergoing schizogenesis, while in the blood taken from the fetal surface there was no such parasite.

In the blood of the umbilical cord there was no malarial parasite. Even in the blood of the placenta of the aborted fetus (six months) there were no malarial parasites.

They go on to say: "The presence in the blood of the maternal placenta of numerous malarial parasites in the act of schizogenesis, demonstrates that these parasites sojourn a long time in the placenta and probably in the vascular lacunæ into which the cotelydons plunge and where the vessels coming from the uterus end. These lacunæ, because of the slowness of the circulation, represent, it appears, one of the places where the parasites take refuge to multiply, but they cannot pass hence into the vessels of the villosities. It is, indeed, probable that some microbes pass through the placenta and infect the fetus, but it would be rash to judge by analogy by admitting that the malarial parasite can pass through the placenta also. For the white cells are, as is known, attracted with eagerness toward the vegetable parasites, and seizing them in the lacunæ where they are found transport them by ameboid movement through the vessels of the villosities, while, with regard to the malarial parasites which are animal, these globules do not appear to be attracted with the same avidity, at least as long as these parasites are living. To this conclusion our observations lead us, for in several thousands of preparations of blood containing malarial parasites which we have examined up to the present, we have seen only a very few which were engulfed by the leukocytes. From all this we can conclude that the opinion of those who think that the malarial parasites do not cross the placenta is correct."

It will be seen that none of the positive cases will stand a rigorous criticism, as in none of them could an extrauterine infection be excluded, the organism having been detected in each instance some days after birth. The negative cases carry more conviction, as in each instance the examination was made at the time of birth and the results have been uniform.

If, then, we accept a denial of intrauterine infection as proven, or, at least, as highly probable, and if from the season of the year and the absence of the anopheles in a given environment we exclude the usual means of an extrauterine infection, a third

method of transmission must be sought to explain such a case as we have under consideration.

Concetti says, apropos of this class of cases: "Only in one case can we conceive of the transmission of malaria from the mother to the fetus, it is in the case of a placental hemorrhage, either during pregnancy or at the moment of delivery. But in this case it is a simple transmission by inoculation, . . . with the absorption of blood from the maternal placenta, rich, as we have seen it, in malarial parasites."

This was the explanation given by Peters to account for the mode of infection in his case, which, in many particulars, was strikingly like my own. This child was born during a malarial paroxysm of the mother. It was healthy at birth. On the fiftieth day after confinement the mother brought the child for treatment and the next day organisms of the tertian type were found in the blood.

The first expression of malarial poisoning in the child was probably in the second week, when the mother said the child "got cold and had fevers." The infant had always been pale, but not enough to cause alarm until a week before she brought it in. It was extremely anemic, the skin slightly yellow and the spleen 3 cm. below the costal margin. The temperature never was above 99.7°. It was cured by quinine, gr. i. *t.i.d.*

Dr. Floyd Crandall reported a case in the New York Polyclinic February, 1903, that seems, indeed, to have been an intrauterine infection. The child was born of a malarial mother, in a precipitate labor and had a paroxysm eighteen hours after birth. The malarial organism was found on the third day. There was no enlargement of the spleen, which, taken with the character of the labor, makes it possible that there had been some disturbance at the placental site, permitting of a mingling of the fetal and maternal bloods and, hence, an inoculation.

Not only does the fetus escape the infection from the mother, but seems not even to share with her the depraved blood and nutritional states from which she suffers. To quote Concetti: "One might say that the fetus enjoys a certain immunity in the presence of the secondary effects of the infection, that is to say, of the intoxication which is the immediate product of it (toxins of malaria)." Pezopoulos and Cardamatis express themselves to the same effect and attribute the fact to a natural resistance on

the part of the child to a malarial parasite and its toxic product.

It will be recalled that in both Peters' case and my own the infants were said to be perfectly healthy at birth.

Concetti says that Valagussa and himself have seen circulating in the blood of certain infants large numbers of benign malarial parasites, without the febrile reaction that might be anticipated. Hence, he argues that infants have an immunity, not against the parasites, but against the toxins.

Koch reports that most of the children of natives of Africa, though they, in appearance, are in perfect health, harbor in their blood the malarial parasite. Pezopoulos and Cardamatis also report finding in the blood of an infant of thirty-five days numerous parasites, though the child seemed in perfect health and never suffered from fevers.

That this immunity does not obtain in all cases and that the infant may be even more susceptible than the adult is shown by Peters' case and my own, the latter suffering from a pernicious type of the disease, in the end proving fatal.

It is generally appreciated that the sequence of events in malarial poisoning differs from that in older children and adults. There is no chill, but rather cyanosis of fingers, lips, ears, nose, or even general drowsiness and prostration, vomiting and often diarrhœa. There is little or no fever, or, if high, of the remittent, rather than intermittent, type. In the case here reported none of these symptoms were noted.

Clinically, then, this picture was not one of malaria, but rather of the symptom complex described by von Jaksch, showing extreme anemia, a very large spleen, an enlarged liver, a blood picture of anemia or lymphatic leukemia of the sublymphemic type, a reaction of the hematopoietic organs, poikilocytosis, anisocytosis, numerous nucleated reds, a few myelocytes and mast cells.

I am sure that, given this picture in the infant six months later, with an absence of the malarial organisms, there would be no hesitation in including this under the designation of anemia pseudoleukemica infantum. To my mind it adds support to the contention of many clinicians that von Jaksch's disease is but the expression of blood destruction and hematopoietic reaction, induced by toxins of varied derivation.

SOME OBSERVATIONS CONCERNING SCARLET FEVER, RUBELLA SCARLATINOSA, AND SO-CALLED DUKES'S DISEASE.

BY D. J. MILTON MILLER, M.D.,
Atlantic City, N. J.
(Visiting Physician to the Children's Hospital, Philadelphia.)

Because of the hackneyed character of my theme, this paper is presented with considerable hesitation. No new facts are adduced that will clear up the confusion still existing in the minds of many practitioners concerning the affections I am about to consider, these observations being recorded solely for the purpose of bringing forward several points connected with the symptoms, diagnosis and identity of scarlet fever and rubella, which have often interested and puzzled me, and doubtless may have interested and puzzled some of those present to-day. I hope I may be pardoned if I introduce my subject by briefly relating some case histories, which, for the purpose of study, have been divided into groups.

GROUP I.

Many years ago I saw a girl of thirteen, who was taken abruptly ill with sore throat and a scarlatinal rash. The throat was bright red, and the tonsils the seat of a yellow exudate. The lingual papillæ (fungiform) were rather prominent, and the tongue coated. The temperature was 102.5°F.; the pulse, 120. The eruption had begun on the chest, ears and upper arms; was composed of minute red dots, or puncta, surrounded by areolæ of lighter hue, and was marvellously like that of scarlet fever. It spread rapidly over the body, but appeared only sparsely below the knees and on the forearms and backs of the hands as discrete red dots or points, very slightly elevated, with areas of normal skin intervening. It was attended with considerable itching, and lasted five days, and was followed by a furfuraceous desquamation, but there was no peeling. On the second day the temperature was 99.5°F. and 101°F. respectively, gradually falling to normal with the disappearance of the eruption. The tongue cleaned off, but did not leave the papillæ particularly enlarged.

There was no initial vomiting, and the child at no time appeared ill. As the patient had never had scarlet fever, I regarded the case as such and acted accordingly. Exactly fourteen days after the initial symptoms of the preceding case, her sister, aged fifteen, who had had scarlet fever severely many years before, and who had not been near the first case since the first hours of the attack, was seized with a precisely similar disorder, viz., exudative tonsillitis, fever (102°F.), scarlatinoid rash (more extensive than her sister's), moderate course, and no desquamation. The day following the development of the second case, a third sister, aged nineteen, who also had not had scarlet fever, became ill with a well-marked exudative tonsillitis, fever (103°F.), no lingual change, absolutely no eruption, and no desquamation. In all the cases the urine was normal. The occurrence of the second case in one who previously had been the subject of scarlet fever caused the diagnosis to be changed to rubella scarlatiniforme, although that exanthem was not present at the time. I was perplexed, however, that in a small house-epidemic of what was apparently *not* scarlet fever, 2 cases should occur with eruption and angina, while a third had the angina alone—an association of phenomena not uncommon in true scarlet fever. The only remaining explanation, viz., that the cases were simply tonsillitis with fortuitous eruption does not seem worthy of consideration. No mention of the condition of the cervical glands is made, because this feature was not noted.

GROUP II.

In the summer of 1895 I observed an attack of eruptive disease in a child of ten, which was the exact counterpart of those just described. This patient, who had never had scarlet fever, nor has had it since, after twelve hours of sore throat, had a densely punctated scarlatinoid efflorescence on the chest, trunk and back. Over the surface of hands and feet the puncta were discrete, consisting of minute, slightly elevated red dots; the body otherwise being well covered. The eruption lasted five days, and disappeared without desquamation. At the onset the temperature was 103°F., the pulse 140, but neither rose during the next and succeeding days above 100° and 99°F. respectively. On both tonsils were patches of yellow exudate. The tongue was very red, but the lingual papillæ were not especially elevated, nor did it desquamate in any peculiar manner. All the cervical glands, anterior and

posterior, were palpable. Initial vomiting was absent. The urine was normal, and the child at no time appeared to be ill. I was, at first, in doubt as to the nature of this case, but the appearance of a similar attack, pronounced scarlet fever by two physicians, in a boy in the same hotel, with whom my patient had played just prior to her illness, led me to isolate the child for four weeks; but desquamation never supervened, although the boy was said to have peeled profusely. Still, as far as my case is concerned, I was never satisfied with the diagnosis of scarlet fever; an uncertainty in a measure confirmed by the discovery that there had been in the neighborhood during the previous spring an extensive epidemic of a mild eruptive disease, which had only in few instances desquamated, and which physicians had variously denominated rose-rash, roseola, scarlet fever, etc. (This case, from the rash, throat, and general symptoms alone could not be separated from mild scarlet fever. It was the absence of desquamation of skin and tongue, and the occurrence of the aforesaid epidemic that made the diagnosis of rubella probable.)

GROUP III.

In the fall of 1899, H. H., aged eleven, who had had scarlet fever some years before, was taken ill with sore throat, vomiting and fever, and in sixteen hours, with a dull red, scarlatinoid, punctated rash, commencing upon the chest and covering the whole body densely, except on the hands and feet, where it appeared as small, closely set, but discrete red dots. On the tonsils was a dirty-yellow exudate, and the papillæ of the tongue were elevated. The cervical glands were palpable, and the constitutional symptoms marked; the temperature, 101° to 103.5° F.; the pulse, 120 to 140. The eruption lasted five days; but although closely watched for five weeks, no decided desquamation of the tongue, and none whatever of the skin, was observed. At the height of the illness the urine contained albumin and a few hyalin and granular casts (febrile albuminuria), but not subsequently. In the second week there was synovitis of both wrist-joints, and during convalescence otitis media and mastoid disease requiring operation. Certainly one would feel justified in calling this case scarlet fever, yet the previous attack of this disease, the total absence of desquamation with a *most pronounced eruption*, and the fact that the patient was subject to severe tonsillitis, caused me to

look upon the attack as one of sepsis, although rubella scarlatinosa or fourth disease, if there be such, cannot be ruled out. (In Curtis's and Shaw's epidemic of scarlatiniform rubella, *Medical News,* December 20, 1902, there were many cases as pronounced, and with complications as severe, as in this one.)

GROUP IV.

None of the children in this group had suffered from, or had been exposed recently to, scarlet fever or rubella. On September 4, 1908, E. T., five years old, awoke with sore throat and some malaise. At noon he had fever and a rash, which at 3 P.M. was discovered to be a punctate scarlatinoid erythema on the chest, back, trunk and arms. There was an exudate on both tonsils, a stippled palate, not very red; no marked engorgement of the lingual papillæ, a temperature of 102.5°F., and a pulse of 150. That evening the temperature was 103°F., and the next day 101°F., with a pulse of 100. On the following and succeeding days the rash spread to the forearms, legs, hands and feet, where it was sparse and inclined to be discrete, as is so frequently observed in scarlet fever. The eruption lasted four days and was attended by considerable itching. The face remained free. The glands posterior to the sternomastoid muscle and back of the ear were palpable. A consultant saw this patient on the third day, and scouted the idea of scarlet fever, regarding the rash as accidental. With considerable reluctance on my part, desquamation not supervening, quarantine was raised on the eighth day; and that night, for some unaccountable reason, the patient's sister, aged three, occupied the same bed with him. In exactly seven days the latter developed a similar train of symptoms, except that there was initial vomiting, *twice repeated*, a more pronounced rash coming out in twelve hours and involving the face, with circumoral pallor and pin-head elevations, generally at the site of the hair follicles ("goose-flesh" appearance); a tongue with enlarged papillæ, desquamating on the third day; enlarged postmastoid and anterior cervical glands, and a temperature, which, although not higher, persisted longer. Simultaneously with the onset of the sister's illness, the baby, eighteen months old, had slight tonsillitis and a temperature of 100°F., a faint scarlatinoid rash on chest and trunk, enlargement of postcervical glands, and no desquamation. At the same time the grandmother, who had had scarlet fever

many years before, and who nursed the baby, also developed tonsillitis without eruption. The boy desquamated in branny scales in the groin only; the girl not at all on the trunk, but on the seventeenth day there was slight peeling from some of the fingers and toes and the palms of the hands.* The urine in all remained normal, except that in the girl a trace of albumin and hyalin and granular casts were found at the onset only (febrile albuminuria). These cases were pronounced scarlet fever, largely because of the desquamating tongue in one, and the short incubation of the second and third cases, viz., seven days from the release of the boy from quarantine, although infection might have occurred *fourteen days earlier,* when the first case developed. Rubella, however, cannot be positively excluded; the postauricular glands were enlarged, and remained so for quite a time, and desquamation, notwithstanding a pronounced rash, was practically wanting.

Although I have designated the probable diagnosis in each of these groups, I do not feel quite positive as to any of them. If all the cases had occurred in one epidemic, there would be little difficulty in asserting, with the exception, perhaps, of Groups I. and III., that they were, at least, not scarlet fever; but happening individually or in small house-epidemics, the proposition is different. That Groups I. and IV. were simply tonsillitis with a scarlatinal erythema is not conceivable; they were clearly instances of one of the infectious exanthemata.

This brings up several points in connection with these affections which I desire briefly to discuss: First, Can scarlatina occur with a pronounced eruption, but without subsequent desquamation? Exfoliation of the epidermis is a feature of this exanthem generally considered as characteristic as the efflorescence; but T. S. Westcott and C. G. Kerley have seen cases without it, and McCollom says not all cases desquamate; consequently, too much stress must not be laid upon *this feature.* Its presence is of less moment than its absence, the latter being of considerable diagnostic value in excluding scarlet fever. The peculiar feature of scarlatinal scaling is its long duration and often late appearance. This point is of material importance, and helped to throw the balance in favor of scarlet fever in my last group. Schamberg believes that in the majority, if not all, of the cases of scarlatina, vesicles are present, and that desquamation is

* Daily inunctions and baths were used in these cases.

directly proportionate to the amount of vesiculation. Trousseau has made the same observation. If this be true, it is not difficult to conceive of cases in which the corium is less deeply affected, hence no vesication and consequent desquamation. On the whole, the evidence seems to confirm the possibility, but infrequency, of a well-marked rash in scarlet fever without desquamation.

Second, Can there be angina in rubella scarlatinosa with no efflorescence upon the skin—a rubella *sine eruptione?* We know that this is observed quite frequently in scarlet fever, but I am acquainted with no observations as to such an occurrence in rubella; yet in my first group, the cases of which were certainly not scarlet fever, and in my fourth, which might have been rubella, this event happened; and as in rubella scarlatinosa an exudative tonsillitis, often quite as severe as in scarlet fever,* may occur, there is no inherent difficulty in believing that in rubella, as in scarlet fever, there may be an angina without eruption as the only manifestation of the disease. If this is impossible, then my first cases were either scarlet fever or tonsillitis—an assumption I am not willing to admit.

Third, Is the strawberry-tongue (*i.e.*, the desquamating tongue with enlarged papillæ) always observed in scarlet fever? Griffith says that he never saw a case without it; Kerley, that it may or may not be present, and is of little value in diagnosis; McCollom, that it is a constant symptom, never absent, though, if slight, it may escape detection; Holt, that although frequently seen, it is not peculiar to the disease. Many of the older writers regard this as the most constant feature of scarlet fever, and pathognomonic in itself; but, without doubt, it is sometimes absent in genuine cases of scarlet fever, and is seen in other conditions;† and, as these conditions may exist in scarlet fever, its mere presence in a scarlatinoid affection is no evidence of scarlet fever. Its presence, however, is of very strong confirmatory proof, more important than its absence, and is of material assistance in the differentiation of these disorders.

In the case which I have cited the question of the ordinary, or morbiliform, type of rubella never arose; nor was the erythema infectiosum of Escherich considered; the diagnosis lay wholly between scarlet fever, rubella scarlatinosa and some other affec-

* See Curtis's and Shaw's account of the epidemic of rubella scarlatinosa at Round Lake, N. Y. (*Medical News*, December 20, 1902.)

† In epithelial desquamation of the tongue—so-called geographical tongue.

tion—the so-called "fourth disease" of Filatow and Dukes. Were my cases, after all, but instances of this possible new exanthem? With the exception of the conspicuous absence of desquamation in most of them, they conformed, barring some minor details, to the description of Dukes and others (Weaver, Simpson, etc.), but they also correspond to varieties of rubella often observed when this disease is epidemic in a scarlatinoid form, as well as to many mild cases of scarlatina. It must be confessed that it is unusual for one affection to exhibit two such distinct types as the two varieties of rubella, the one very much like measles, the other closely resembling scarlet fever. No such distinct types are found in the other eruptive diseases, yet the occurrence of epidemics in which one type alone is seen, or of epidemics of one or the other type in which the opposite type is observed, seems to show that such wide variations in one disease are not only possible, but do occur.

Although it would be interesting, it is not necessary to enter into a critical analysis of Dukes's claims. This has already been done in so masterly a manner by Griffith and Ker that I think the latter's verdict of "not proven" must, for the present, at least, be accepted. Until it can be shown that an attack of "fourth disease" protects against rubella, and yet occurs in those who previously or subsequently have had scarlet fever, the existence of a distinct and new exanthem must be denied. As yet, no such evidence has been forthcoming, and I am fully in accord with Griffith that the recognition of a new exanthem would be little short of a calamity, and would but tend to make "confusion worse confounded." With our present knowledge, it is safer to regard these specific scarlatinoid exanthems as mild scarlet fever, as many of them undoubtedly are, or as rubella scarlatinosa, as some of them also are, at the same time keeping our minds free to the possibility of a new disease, or to the old doctrine of the hybrid nature of German measles, having in mind Goodhart's contention that there is no inherent objection to such a conception: "Plants and animals vary their type under domestication; why may not disease germs be subject to similar variations? There is really no essential difficulty in regarding rubella as a hybrid, and none in regarding it as a distinct, but, as yet, unstable species."

Diagnosis.—In the differentiation of rubella scarlatinosa from scarlet fever, an observation made by the writer some years ago

has appeared, at times, to be of value. I refer to a prolonged prodromal period in rötheln. This, in my experience, exists more frequently than is generally supposed. I have often observed distinct, but slight, prodromes of two or three—exceptionally five—days' duration, consisting of malaise, mild catarrhal symptoms, or enlargement of the postmastoid glands. T. S. Westcott informs me that he, also, has found this feature of some value in diagnosis. (This may exist as the only prodromal symptom for several days before the appearance of the eruption.) The fact that the eruption of scarlatina, as a rule, avoids the face, while the converse obtains in rubella, is of no value in diagnosis, for in some epidemics of rubella scarlatinosa, as in scarlet fever, the face is apt to remain free. The question of the possibility of second attacks of scarlet fever inevitably arises in a discussion of this character. Scarlet fever was ruled out in the first, and, especially, in the third group of cases, because of the history of a previous attack. Second attacks undoubtedly occur, but it is significant that they were more frequent when the identity of the various scarlatiniform erythemata, as well as that of rubella scarlatinosa, was not as completely understood as it is now. Only cases supported by the strongest evidence should be accepted as genuine. So difficult and often impossible is it to distinguish between rubella scarlatinosa and scarlet fever, and mistakes may have such grave consequences, that it behooves us to be very slow in making a diagnosis of rubella. Students and others should be taught that German measles is a rare disease, scarlet fever a common one, and that the custom of calling every mild scarlatinal eruption roseola, German measles, etc., is a bad one, and is fruitful in disseminating the more serious affection. This is the opinion of all who have had opportunity of seeing large numbers of cases of the infectious exanthemata. Too much stress cannot be laid upon the fact that scarlet fever may occur with the mildest constitutional symptoms. Williams (*British Medical Journal*, No. 2,138, 1901) has reported an epidemic of scarlet fever resembling rubella scarlatinosa in which there was no vomiting, a low pulse-rate, no lingual desquamation, and no renal complications. Many of these cases were treated in scarlet fever wards, but none contracted scarlet fever. My own mishaps have taught me that in cases of doubt one should act as if the more serious disease were present; and when we meet with cases which, in the absence of an epidemic, we cannot explain satisfactorily, to treat them as

scarlet fever until they do or do not desquamate, even if considerable inconvenience and unfavorable criticism are evolved thereby. The public and the physician, as well as the patient, should have the benefit of the doubt. Rubella scarlatinosa should be admitted only on the strongest evidence, and lack of well-marked signs, or of any of the usual signs of scarlet fever, as all of us know, is not sufficient to exclude this exanthem.

Finally, I would call attention to the mildness of the scarlet fever prevailing at the present day, and to the well-known fact that there have been periods in its history in which it was considered a very trivial affection (Sydenham), just as rötheln is now, and that, on this account, its contagiousness was at one time actually denied (Chapman, 1833-36). Is it possible that we are passing through or entering upon a similar epoch in the history of the disease, and that this explains many cases of rubella scarlatinosa and the question as to the possibility of a "fourth disease"?

DISCUSSION.

DR. KOPLIK.—As to the question of second attacks of scarlet fever, I recently had to look up this matter and gained the impression from the literature that such attacks are quite possible, though rare.

As to scarlet fever, there are recorded cases of a very mild nature, and recently in New York there occurred a number of cases that were very mild in character which closely resembled röthlen. The temperature rarely ran above 100° F. or 100.5° F. A mild rash appeared, but left nothing behind. The patients continued comparatively well until the tenth day, when the cervical glands enlarged and the child became quite ill, running high temperatures, and this continued for a week or more. In one case the patient had an evanescent rash, and on the tenth day developed enlarged glands, and a week later went into a condition of convulsions with suppression of urine. I came to the conclusion that these cases were cases of mild scarlet. Though the rash was very mild the subsequent course was not so mild. I agree with Dr. Miller, that in practice we should assume that a case is scarlet, no matter how mild the eruption, until we have proven otherwise by isolation and watching for several weeks. In the cases I mention, quite a number were in my own practice, and they developed a fine branny desquamation sometimes three weeks after the eruption.

As to the postcervical glands in röthlen, I have seen and described them. A patient once asked me why the glands in the

back of her neck were enlarged five or six days before the development of this disease.

DR. SHAW.—In regard to the epidemic quoted by the writer, Dr. Curtis, who saw the cases with me, is a man who has had as much experience perhaps as any one in the state of New York outside of New York City. There was not a shadow of doubt in our minds that we were dealing with an epidemic of rubella scarlatinosa. This little village of Round Lake is a summer resort, with, in winter, about four or five hundred population. To have been quarantined would have worked a hardship upon these people. A peculiar feature was the endemic character. A large majority, over 40 per cent. of the population, contracted this eruptive disease. It was more prevalent among adults than among the children. The rash was typical and in two or three weeks there was desquamation. The incubation period was twenty-one days. The local doctor had treated quite a number of children and adults for scarlet fever in previous years. Over 20 of these people had had scarlet fever to his knowledge. The condition of the entire lymphatic system was striking. The postcervical glands were enlarged in every case, and persisted for six or eight weeks.

I was recently called in to see a boy of fifteen with a very acute rash and found that he had had four other attacks since going to school. Each time he has been quarantined and kept out of school.

DR. ADAMS.—I would like to ask how many have seen, or actually attended, cases of scarlet fever in the second attack? Of course, it is possible for an individual to have a second attack, but my experience has been that when I made a diagnosis of scarlet fever the mother has told me that the child has had the disease before, but after the disease was well developed then she would say, Well, the last time it was not like this. I once asked an old practitioner, who had been practising in Sheppardstown, Va., for over fifty years, if he had ever attended a case of scarlet fever the second time, and he said no, but that he had attended cases that had said that they had had scarlet fever before. Very frequently we have cases in which the history would seem to be that the child has had scarlet fever and it would be interesting to me to know of authentic cases of second attacks. I have attended people for over thirty years and have not seen a second attack in the same individual. We cannot place any reliance in statements of the family. They may make the statements to protect themselves from quarantine.

DR. FREEMAN.—It seems to me in a questionable diagnosis between scarlet fever and German measles it is important that the cases be treated as if they were scarlet fever. We had at the Foundling Hospital a few years ago an epidemic of some 8 or 10 cases occurring at the same time, none of which had a tempera-

ture over 101.5°F., and with slight rash, and yet almost all of them had bad complications, nephritis, bad joints and suppuration of the glands of the neck.

DR. ROTCH.—I have never seen a recurrent scarlet fever in my own practice. I should say it is quite rare.

DR. ABT.—I have diagnosed scarlet fever twice in the same individual. Six or seven months ago I saw a little boy with one of these nondescript rashes and felt called upon to call it scarlet fever. About five months later he came down with a distinct scarlatina and finally desquamated. Previously there was some doubt as to whether he had desquamated or not. We are not always in a position to make a definite diagnosis, and I am in accord with Dr. Miller that these doubtful cases which closely resemble scarlet fever should be regarded as cases of scarlet.

I should like to ask Dr. Shaw if in the epidemic he refers to there could have been any relation between it and the milk supply. In a suburb of Chicago quite a distinct epidemic was traced to one of the milk supplies, where several of the employees were actually desquamating.

DR. SHAW.—The question of the milk supply was one of the first things considered, and while we found that the majority of the people had their own cows, yet breast-fed infants were not infected and nearly all the families supplied by the one milkman contracted the disease. In this epidemic there were no complications and no deaths.

DR. SOUTHWORTH.—The more I see of the eruptive diseases the more hesitation I have in making an immediate diagnosis on account of the infinite variety of eruptions that occur in children, but I believe that all such cases should be quarantined until we can be absolutely sure. As one's experience becomes larger he finds great variations from the cut and dried classifications of the text-books.

DR. PUTNAM.—In the case where no desquamation occurred did the doctor examine the bit of dry skin at the back of the nail? I had a case under close observation of a girl known to have been exposed to a well-marked case. She developed a slight rash and a slight sore throat. The tongue could not be called a strawberry tongue. Next day there was absolutely nothing to be seen. She was kept isolated several weeks, but at last went to the country home, and in a short time another child of the family came out with a perfectly plain case of scarlet fever. I went down to examine her again and found nothing but a slight translucency of this bit of cuticle at the back of the nail to which I have referred, yet I feel sure she had scarlet fever.

DR. HAND.—As to the importance of the diagnosis between scarlet fever and rubella, it reminds me of a position I was in last

winter. A child developed a rash the day before Christmas, with fever and rapid pulse. The throat was a little red. The rash was such that I had to call it scarlet fever, and when I announced the diagnosis the mother said, "It could not have been at a worse time, as the room is full of Christmas presents being sent out to friends." If I had not called it scarlet fever those presents would have been sent out. In a few days there was nothing to be seen at all, but three weeks later there was desquamation around the finger-nails, to which Dr. Putnam has just referred.

I do not attach much importance to the absence of desquamation on the body in private practice, as it is my custom to have the patient receive a daily inunction of oil or carbolized vaselin.

DR. GRIFFITH.—I have seen two undoubted attacks of scarlet fever in the same patient, and shall report the case at a future time.

DR. MILLER (closing remarks).—As regards second attacks of scarlet fever, I take the stand that there are so few authentic that I do not think they should be considered. My object was to try to show that there is a disease—scarlatinous rubella—which so closely resembles scarlet fever that in isolated cases it is impossible to make the diagnosis, and that rubella scarlatinosa occurs both epidemically and in isolated cases, and that all such cases should be treated as scarlet fever for the protection of the public and the patient. I believe that some cases of mild scarlatina are in reality rubella scarlatinosa, and the converse of this is also true, so that we should be very careful in diagnosing rubella.

THE URINE IN THE GASTROINTESTINAL DISEASES OF INFANCY. AN ANALYSIS OF THE RESULTS OF THE EXAMINATION OF THE URINE IN THREE HUNDRED CASES.

BY JOHN LOVETT MORSE, A.M., M.D.,

Assistant Professor of Pediatrics, Harvard Medical School; Associate Visiting Physician at the Children's and Infants' Hospital; Visiting Physician at the Floating Hospital, Boston.

AND

BRONSON CROTHERS, A.B.,

Boston.

These babies were patients on Dr. Morse's services at the Infants' Hospital and Floating Hospital. They were consecutive, except that in many instances, especially at the Floating Hospital, the urine for one reason or another was not examined. In most instances but one examination was made. In many, however, especially if anything was found wrong at the first examination, repeated tests were made. The examination was usually made within the first forty-eight hours after entrance to the hospital. The test for albumin was made either with cold nitric acid or with acetic acid and heat, sometimes with both. The sediment was usually not examined unless albumin was found. Unfortunately it was sometimes not examined even when there was albumin. The writers realize that examinations made in this routine way, and not repeated, do not justify any very definite conclusions. It seemed to them, however, that, on account of the large number of babies studied, even such incomplete methods ought to give a general idea as to the frequency of albuminuria and renal complications in these diseases. They also hoped that they would give some idea as to the importance of the urinary findings in prognosis and that they might show something as to the relation between some of the other symptoms, such as stupor, restlessness and edema, and the urinary findings. The subject has been taken up entirely from the clinical side, as autopsies were so few and so incomplete that they showed nothing as to the pathologic conditions in the kidneys.

The literature of this subject is fairly voluminous and was reviewed by one of us (Morse) in papers published in 1899 and 1902. Koplik's paper, published in 1899, as well as several of those published since 1902, are, however, worthy of special consideration in this connection.

Koplik found evidences of inflammation of the kidneys in 21 of 25 consecutive cases of acute or subacute gastroenteritis in infants. He believes that a renal complication adds certain distinctive symptoms to the picture of the disease, and does not agree with the French writers who claim that they have no characteristic symptoms. He thinks that the symptoms from a complicating nephritis can be divided into two classes—the first general and inseparable from the primary disease, the second directly traceable to the affection of the kidneys. In the milder cases there are usually no symptoms; in the severer forms there is often marked restlessness, sometimes alternating with periods of stupor. He believes that persistent vomiting, which is not relieved by one or two irrigations of the stomach, is due to a disturbance of the function of the kidneys and a manifestation of uremia rather than of toxemia. He also believes that the edema sometimes seen in these cases, most often in the lower extremities, is due to trouble in the kidneys. He feels very strongly that these symptoms, which have usually been attributed to toxemia rather than to uremia, are uremic and not toxic. He believes that the lethal ending is hastened by the renal complication, but thinks that with proper treatment the prognosis is in general good.

Hohlfeld studied very carefully the urine of 40 babies suffering from various diseases of the gastroenteric tract. Five of these had chronic gastrointestinal catarrh, 3 of whom died. A trace of albumin was found in 4; the sediment was slight, but contained casts and cells and very rarely blood. Sixteen had chronic catarrh of the colon and all died. He found albumin in the majority all of the time. The sediment varied with the amount of albumin; as a rule, there was considerable, its composition being the same as in the cases of chronic gastrointestinal catarrh. He often found casts when there was no albumin. Sixteen had acute catarrh of the colon, 13 of whom died. He found albumin in all but one. This one died in twenty-four hours, only one examination of the urine having been made. The sediment in these cases was macroscopically slight, but micro-

scopically very rich. It contained very many hyalin casts, but few of other varieties, numerous cells and leukocytes and very little blood. Three had cholera infantum, all of whom died. The urine contained much albumin. The sediment was abundant, the casts more varied than in the cases of catarrh of the colon, and the sediment showed more fat and very little blood. He also believes that the restlessness, stupor and vomiting in these cases are symptoms of uremia, but admits that they may also be explained by the toxemia, the feeble circulation and the loss of fluid. He had autopsies in 35 cases. The changes found in the kidneys were mainly of the parenchymatous type; no case showed a pure interstitial nephritis. In 2 cases a peculiar condition of infarction dominated the picture and once there was a thrombosis of a renal vein and a hemorrhagic infarction of a whole kidney. The pathologic changes seen in these cases were those ordinarily found in intoxications and infections, both of which conditions are present. They must be due to the absorption of toxic substances from the intestine, because bacteria are not found in the kidney. He believes that the disturbance of the eliminative function of the kidneys and the formation of toxic substances in the gastrointestinal tract react on each other and render the prognosis unfavorable. It is not absolutely bad, however, as some get well. A resulting chronic nephritis is possible, but not probable. He thinks that salt solution given subcutaneously is the best treatment for the renal complications.

Pick carefully examined the urine of 36 babies ill with various diseases of the gastroenteric tract. He found albumin and casts in the urine of 2 cases of dyspepsia and in 3 of acute intestinal catarrh. The sediment in one of the latter contained red corpuscles. One of them had edema; none showed nervous symptoms. One of each class died; 1 of the dyspeptic cases was discharged unrelieved; the other 2 recovered. He found albumin in varying amounts in most of 23 cases of cholera infantum. The sediment always contained casts, sometimes blood and occasionally bacteria. He found casts even when there was no albumin. There was edema in some cases and not in others. The amount of edema had no relation to the presence or absence of albumin. He was unable to determine whether the nervous symptoms were due to uremia, toxemia, or enfeebled circulation.

Fourteen died, 7 were discharged unrelieved and 2 recovered. The kidneys showed congestion, fatty degeneration of the epithelium of the straight and convoluted tubules, local necroses of the epithelium of the convoluted tubules and foci of recent interstitial inflammation in the cortex. Four cases of subacute intestinal catarrh all showed a great deal of albumin, many casts, fat and so on. They had edema, but no other symptoms of uremia. Three died and 1 recovered. The worst one recovered and the urine was normal four and one-half months later. The urine of the 4 cases of chronic intestinal catarrh never showed much albumin, seldom casts and never blood. None of them had uremic symptoms. Three died and 1 recovered. There was marked fatty degeneration of the epithelium of the convoluted and straight tubules. The glomeruli were normal and there were no interstitial changes. The symptoms of nephritis were slight in all these cases. He believes that the edema in these cases may or may not be due to nephritis; that the symptoms which are often thought to be uremic may be due to toxemia or disturbance of the circulation and that toxemia is the most important cause of the nervous symptoms.

Chapin examined the urine of 86 babies suffering from various gastroenteric disturbances. He found albumin in 75; a faint trace in 31, a trace in 29, and a heavy trace in 15. Casts of various sorts, hyalin, granular, epithelial and mucous, were found in 37. Sixteen died, 14 of whom had albuminuria and 10 casts as well.

Knox and Meakins examined the urine of 53 babies ill with diarrheal diseases. Thirty-three had dyspepsia or fermental diarrhea, and 20 ileocolitis, the division being based on the character of the dejections. The urine was abnormal in 22. The urine contained albumin alone in 2 and albumin and hyalin casts in 5, a finding which they believe indicates cloudy swelling of the kidneys. It contained pus alone in 2 and pus and albumin in 5, a finding which they consider characteristic of pyelitis. It contained pus, albumin and casts in 4; pus, albumin, casts and many bacteria in 2; pus, albumin, casts and red blood cells in 2, a finding which they consider represents nephritis or pyelonephritis. The urine was abnormal in 30 per cent. of the cases with dyspeptic diarrhea and 60 per cent. of those with ileocolitis. The number

of cases with albuminuria, pyelitis and nephritis was essentially the same in both classes, the affection of the kidneys being about as severe in one class as in the other. Twenty-seven per cent. of those in which the urine was abnormal died or were discharged unimproved, as compared with 19 per cent. of those in which the urine was normal; that is, the general result was better in the cases in which the urine was normal. They cite 2 cases in which the urinary abnormalities did not hinder recovery, and one in which the urinary complication was the chief ailment.

The classification of the diseases of the gastroenteric tract used in this paper is that adopted by the Pediatric Department of the Harvard Medical School. (See Rotch, ARCHIVES OF PEDIATRICS, 1906, Vol. XXIII., p. 881.) The urine of 300 babies, as already stated, was examined.

Regulation of Feeding.—In 39 cases the disturbances of the gastroenteric tract were not very marked, so that no more definite diagnosis than regulation of feeding was made. All of these babies were under one year old and were discharged well. A slight trace of albumin was found in one. The sediment in this case showed nothing abnormal.

Chronic Gastric Indigestion.—Albumin was found in the urine of but 2, or 4 per cent., of 53 cases. Five, or 10 per cent., of those in which there was no albumin died. The 2 whose urine contained albumin were discharged relieved. In 1 the urine was high, acid, and contained a very slight trace of albumin. The sediment was not examined. Three days later the urine contained no albumin. In the other the urine was slightly turbid, acid, and contained a slight trace of albumin. The sediment was not examined. One baby whose urine did not contain albumin one and five days before death became markedly edematous during the last twenty-four hours. The temperature was practically normal in all the cases. Vomiting was, of course, a common symptom.

Chronic Intestinal Indigestion.—The urine of 7, or 11 per cent., of 61 babies ill with chronic intestinal indigestion contained albumin; 11, or 20 per cent., of the babies whose urine did not contain albumin died, compared with 2, or 28 per cent., of those whose urine contained albumin. The following table shows the important characteristics of the urine and the final results in the individual cases:—

TABLE I.

No.	Color	Reaction	Sp. Gr.	Albumin	Sediment	Result
1	Pale, turbid	.....	.....	Slight possible trace. Three subsequent examinations; 3, 4 and 5 weeks later showed nothing abnormal.	Numerous red corpuscles; occasional leukocyte and round cell; no casts	Relieved
2	Slight, turbid	Acid	1020	Slight trace	Numerous neck of bladder cells; few leukocytes; no red cells or casts	Not relieved. A. A.
3	Pale, turbid	Acid	1007	Slight trace	Numerous large and small round cells; no casts	Not relieved. A. A.
4	Pale, turbid	.....	.....	Slight trace	Occasional red cell, round cell, leukocyte and granular cast	Relieved
5	Pale	Acid	.....	Slight trace	No real elements	Relieved
6	Normal, cloudy	Acid	.....	Trace	No cells or casts; urates +	Dead, 1 day
7	Clear	Acid	1020	Trace	Not examined	Dead, 2 days

The temperature of all the babies was practically always normal, whether the urine did or did not contain albumin. Repeated examinations were made in some instances both when there was and was not fever, but albumin was never found. In others in whom albumin was never found the examinations were repeated many times over periods of several months. There was no vomiting at the time of or immediately preceding the examination in 65 per cent. of the babies without albuminuria and in 71 per cent. of those with albuminuria. The facts that albumin was always absent in some instances when the amount of vomiting varied materially from time to time and that it was always absent in others, both when there was and was not vomiting, show that there was no connection between the vomiting and the presence or absence of albumin in the urine. One of the babies with albuminuria developed edema of the feet and hands three days after entrance. The urine showed only a slight trace of albumin and the sediment contained no red cells or casts, merely caudate cells and a few leukocytes. There was, therefore, apparently no connection between the albuminuria and the edema, which was much more easily explained by the generally miserable condition of the baby.

Acute Gastric Indigestion.—Albuminuria was found in 3, or 18 per cent., of 17 babies ill with acute gastric indigestion. There was the slightest possible trace in 1, and a slight trace in the

others. The sediment was not examined in 2 and contained only urates in the other. The temperature was more or less elevated in those in which albuminuria was found, and varied between normal and 105°F. in those in which albumin was not found. All the babies recovered.

Acute Intestinal Indigestion.—Albumin was found in the urine of 4, or 6 per cent., of 70 babies ill with acute intestinal indigestion. There was the slightest possible trace in 2 instances, a very slight trace in 1 and a trace in the other. There was no sediment in 1 and it was negative in another. In the third it showed a few bladder cells and many bacteria; in the fourth a few leukocytes, squamous and round cells and a considerable amount of normal blood, but no casts. This baby died seven days later. The urine was turbid in 15 per cent. of the cases without albuminuria. The sediment was usually not examined under these circumstances, but when it was, the turbidity was most often due to an excess of phosphates. The relative duration of the illness before the examination of the urine was essentially the same in both those with and without albuminuria. Seven, or 11 per cent., of the babies without albuminuria, and 1, or 25 per cent., of those with albuminuria, died. The time of the examination before death was, in those without albuminuria, one day, three days, three days, five days, six days, twelve days, and twenty days, and in that with albuminuria seven days.

All the babies with albuminuria vomited, while 54 per cent. of those without albuminuria did not vomit at all. The severity of the vomiting, however, was no greater in those with albuminuria than in many of those without it. None of the babies, whether they did or did not have albuminuria, had any edema. One of the babies without albuminuria was very restless, another had convulsions and 2 others were in a condition of moderate stupor. None of these symptoms was present in those with albuminuria. Marked prostration was a common symptom in many of those whose urine was normal and was noted in one of those with albuminuria. The temperature was on the average no higher in the babies with albuminuria than in those without it.

Infectious Diarrhea.—Albumin was found in the urine of 10, or 16 per cent., of 61 babies ill with infectious diarrhea. The amount of albumin and the nature of the sediment, as well as the final results, are shown in the following table:—

TABLE II.

No.	Duration	Color	Reaction	Sp. Gr.	Albumin	Sediment	Result
1	14 days	Normal	Acid	....	Slight, possible trace	Not examined	Well
2	15 days	Pale, cloudy	"	1002	Slight, possible trace	None	
	24 days	Pale, opaque	"	1008	None	Leukocytes; epithelial cells; bacteria	Dead in 2 days
3	14 days	Pale, clear	"	....	None	Not examined	
	21 days	Pale, turbid	"	....	Slight, possible trace	Many small round cells; few leukocytes; occasional hyalin casts	Not relieved
4	14 days	Normal	"	....	Very slight trace	Very slight; few squamous and small round cells	Dead in 2 weeks
5	21 days	Normal	"	....	Very slight trace	None	Dead in 4 days
6	9 days	Normal	"	....	Very slight trace	Slight; few fatty small round cells; free fat globules	Dead in 10 days
7	?	Normal	"	....	Very slight trace	Few squamous cells; occasional small round cell; rare red blood cell	
	8 days later	High	"	....	None	Not examined	Dead same day
8	21 days	Pale	"	....	Slight trace	Negative	Relieved
9	21 days	Pale, cloudy	1 "	1011	Trace	Few small round and squamous cells; no fat, blood or pus; numerous hyalin casts with round cells adherent; Some epithelial casts	Dead in 3 weeks
10	19 days	Normal	"	....	Trace	Not examined	Dead in 1 day

The urine was often turbid or cloudy when there was no albuminuria. The turbidity in these instances was usually due to urates or phosphates, most often to phosphates. Very many bacilli with a few squamous and round cells were found in 2, and many pus cells in another urine in which there was no albumin. The sediment was usually not examined when no albumin was found. If it had been, it is probable that more cases of bacilluria and pyuria would have been found. Twenty-one, or 41 per cent., of the babies without albumin, and 7, or 70 per cent., of those with albumin in the urine died. The duration of the disease at the time of the examinations of the urine in both those which did and did not contain albumin was essentially the same; so also was the date of the last examination in the fatal cases. All the babies with albuminuria had fever at, or immediately preceding, the time of the examination, while 27 per cent. of those without albuminuria had none. The average temperature of the babies with albuminuria was also higher than that of those with-

out albuminuria. No baby of either class had any edema, although one without albuminuria had a marked angioneurotic edema. Four, or 8 per cent., of the babies without albuminuria were very restless. This symptom was not noted in any of those with albuminuria. Slight stupor was noted in 3, moderate in 2, and marked in 2, or 14 per cent., of the babies without albuminuria, while 3, or 30 per cent., of those with albuminuria were markedly stuporous. The following facts show, however, that there could not have been any direct connection between the stupor and the albuminuria. In one instance the stupor was only moderate at a time when there was a very slight trace of albumin and an occasional renal and blood cell in the urine, while it was very marked on the day of death, a week later, when the urine showed nothing abnormal. In 2 others the stupor was marked, although the urine contained only the slightest possible trace of albumin and there was no sediment. Fifteen per cent. of the babies with albuminuria and 54 per cent. of those without it had not vomited for some time before the examination of the urine. On the other hand, a number of the babies without albuminuria had been vomiting a great deal, while none of those with it had been doing so. Moreover, in two instances the vomiting was the same when the urine did and did not contain albumin, the examinations having been made eight days apart. In another the vomiting had almost entirely ceased before death, three weeks later. It seems evident, therefore, that there was no connection between the vomiting and the albuminuria.

SUMMARY.—A review of these figures shows that albuminuria was found in but 8 per cent. of the babies suffering from chronic disturbances of digestion. There was never more than a trace of albumin and it was almost never accompanied by casts. The sediment usually showed only a few cells of one sort or another, thus suggesting some affection of the bladder or pelvis of the kidney rather than of the renal tissues. The albuminuria was never accompanied by edema, and vomiting was no more common in the babies with albuminuria than in those without it. The presence of albuminuria had no effect on the prognosis.

Albuminuria was a little more common in the babies suffering from the acute than in those from the chronic diseases of the gastroenteric tract, being present in a little more than 10 per cent. There was, however, never more than a trace of albumin, and this was not, as a rule, accompanied by casts or blood. If

so, the diarrhea was almost always of the infectious type. The sediment more often consisted of leukocytes and cells, thus suggesting an affection of the bladder and pelvis, as in the chronic cases. The presence of albuminuria had no effect on the prognosis in the acute gastric cases. The mortality was, however, considerably higher in the intestinal cases with albumin than in those without it. Vomiting was more common in the babies with albuminuria, but was no more severe in type. The albuminuria was not accompanied by edema in any instance. Symptoms of disturbance of the nervous system, such as restlessness, convulsions and stupor, were possibly a little more common in the babies with albuminuria than in the others. There was, however, nothing to suggest any direct connection between these nervous symptoms and the albuminuria.

Conclusions.—The conclusions which seem to be warranted by this purely clinical study are as follows: Albuminuria is found in from 8 per cent. to 10 per cent. of babies ill with various diseases of the gastroenteric tract. It is found somewhat more often in the acute than in the chronic diseases. Judging from the examination of the sediment, the pathologic condition in the kidneys almost never progresses beyond that of acute degenerative nephritis (often known as cloudy swelling or active hyperemia). Infection of the urinary tract, resulting in the symptom-complex commonly known as pyelitis, or pyelonephritis, is far more common than acute parenchymatous or interstitial nephritis. In the chronic diseases there is no relation between the albuminuria and the mortality or symptomatology. The mortality is higher in the acute diseases when albuminuria is present than when it is not. The presence of albuminuria in an individual case does not, however, materially modify the prognosis in that case. Edema occurring in the course of diseases of the gastroenteric tract is not connected with the condition of the kidneys. There is nothing to suggest any causative relation between the affection of the kidneys and such symptoms as restlessness, convulsions and stupor. These are due to toxemia and not to uremia.

REFERENCES.

Chapin. ARCHIVES OF PEDIATRICS, 1906, XXIII., 329.
Hohlfeld. Deutsches Archiv. f. klin. Med., 1902, LXXIV., 418.
Knox and Meakins. Arch. of Internal Medicine, 1908, October.
Koplik. New York Medical Record, 1899, LV., 451.
Morse. ARCHIVES OF PEDIATRICS, 1899, XVI., 649.
Medical News, 1902, LXXX., 1071, 1,103.
Pick. Archiv. f. Kinderheilkunde, 1905, XL., 290.

DISCUSSION.

DR. CHURCHILL.—Was the omission of the microscopic examination intentional or because it was thought safe to ignore it? In some cases this is important even when there is no albumin found. In one or two cases I have found a considerable number of leukocytes and other cells, indicating considerable renal irritation, though albumin has not been found later on in the course of the disease. I have always taught my students that the microscopic examination of the sediment should be made whether albumin is present or not.

DR. KOPLIK.—I agree with Dr. Morse that it is difficult to fix the exact significance in these cases of the albuminuria and nephritis. The condition seems to me to be simply one of the manifestations of the general toxemia. In some cases the amount of casts and albumin is striking. Many of these investigations were carried on at a time when the children were much sicker from summer diarrhea than we now see them. True cholera infantum is not so frequent at the present day because of the improvement, even among the poor, in methods of feeding and the handling of the food.

DR. MILLER.—For some time it has been my observation that in the cases of continued and acute diarrhea seen at the hospital there has been found a small amount of albumin without any apparent nephritis during the course of the disease. I believe that it is of no significance whatever, and is due, as Dr. Morse has said, to the general toxemia. The presence of albumin and casts in typhoid fever, and other infectious febrile states, is a very common phenomenon, more so, I think, than recorded investigations would have us believe, but it is of no significance, except as showing the intensity of the toxemia; and this is true, also, of the diarrheas of infancy.

DR. MORSE (closing remarks).—I agree with Dr. Churchill as to the importance of the microscopic examination of the sediment. The sediments were not examined in these cases because the urine was examined merely as a part of the routine work in the hospitals and there was no intention at the time of using them in this connection. In one way it makes the results of the paper more unbiased.

Dr. Koplik is almost certainly right in attributing the variations in the results obtained by different observers to differences in the classes of cases studied. I do not see, however, why I should not have found as much trouble with the kidneys as has any one else, because a large proportion of these cases were at the Floating Hospital, which is the dump for all the bad cases of Boston and vicinity. I do not think any one can have a worse set of infants than we get there.

THE SOY BEAN IN INFANT FEEDING. PRELIMINARY REPORT.

BY JOHN RUHRÄH, M.D.,
Baltimore, Md.

The soy bean (glycine hispida), sometimes incorrectly called the soja bean, is an annual leguminous plant which originally grew in a wild state from Cochin China to the south of Japan and to Java.

From the very earliest time it has been cultivated, especially in Japan and also in China, and it is supposed to have been used for food in China long before the time of Confucius. Although it has been used very extensively in China and Japan it has been very slow in spreading to other lands, possibly owing to the more or less complete isolation of these countries, until a very few years ago. It gradually found its way into India and was brought to Europe something over one hundred years ago, where it was grown in the botanic gardens as a curiosity. In England it was introduced about 1790, but no studies were made of its value from an economic standpoint. In 1875 Professor Haberlandt began a series of investigations with this plant in Austro-Hungary, and in his work published in 1878 he urges the importance of the soy bean as a food both for man and animals. After his death, which occurred in 1878, very little notice was taken of the soy bean in Hungary and the prophecy that he made for its future failed.

The soy bean has been known in the United States for many years. As early as 1829 Thomas Nuttall wrote an article in the *New England Farmer* concerning the bean as a valuable crop for this country. The Perry expedition to Japan in 1853 also brought back soy beans, but until the last fifteen or twenty years the plant was known only as a curiosity.

Recently studies have been made by the various agricultural experiment stations, most notably by those of Kansas and Massachusetts, and since 1898 the Office of Seed and Plant Introduction of the United States Department of Agriculture has taken up the work of distributing the bean. Some private seedsmen have

also recognized the value of this crop and there are a few places where the seeds may now be bought.

The plant is grown in America, but is used chiefly for the purpose of a forage crop and comparatively little reference has been made to its use as food for man. The plant itself is an erect annual with branching stems covered with short hairs. The leaves are trifoliate and also more or less hairy. The flowers are violet or pale lilac in color and are small. The pods contain from two to five seeds and like the stems are covered with stiff, reddish hairs. The different varieties of the bean vary in size, shape, color and in the length of time they take to mature. Some are whitish, some yellow, some are greenish yellow, some brown and some black. In shape some are spherical and some more or less compressed. The plants reach the height of from two to four feet. They bear a remarkable number of beans and the flowers are self-pollinated, making the yield independent of insects.

The bean may be easily grown in Maryland. I am indebted to three friends for experimenting with this plant in their gardens and obtaining good crops. The plant grows best in about the same temperature as corn and when grown for food should probably be handled in the same manner as ordinary field beans.

The varieties differ in the length of time they take to mature. In the south the variety taking a long time to ripen may be grown, and only the more rapid varieties should be attempted in the north. At the present time there are seven varieties handled by seedsmen, and some twenty-two distinct varieties are known. In China and Japan almost every village has its own strain of soy beans. The varieties vary in size, as to the amount of foliage and number of beans produced. In the south the mammoth yellow is much grown, and it yields a large crop of hay and a fair number of medium-sized beans. It may be grown to advantage in poor soil, but it takes a long time to develop. The beans from this variety have been used in infant feeding experiments as well as the next two varieties, the Hollybrook and the Ito San. Both of these may be easily grown in Maryland and further north, and the crop of beans from both is very satisfactory. The other varieties are the Guelph (green), the Samarrow (green), the Ogemaw (brown), and the Buckshot (black). All of these latter may be grown in the north.

The plant assimilates free nitrogen from the atmosphere, and under favorable circumstances develops nodules on the roots like

many other legumes, and this is facilitated by either inoculating the beans or the ground with the bacteria which causes nodules. The bean is best sown in drills and the rows should be wide apart, as otherwise the leaves will form such a dense shade as to interfere with the ripening of the beans. They should be harvested early, as the pods open spontaneously and if allowed to stay on the plants will scatter the beans on the ground. Ripening occurs in the ordinary varieties in from 75 to 90 days. The beans in the pods should be allowed to dry, care being taken to prevent moulding, and they should be carefully protected from rats and mice. As soon as the pods are dry the beans may be easily thrashed out. A full account of the plant will be found in the *Farmer's Bulletin*, No. 58, of the United States Department of Agriculture, and of the different varieties in the *Plant Industry Bulletin*, No. 98. I am indebted to Mr. Frank N. Meyer, agricultural explorer for the Department, for information concerning the use of the beans in the East.

The soy bean has been used to improve the quality of the soil and for silage and also as a hay crop. The beans have been used to feed animals, more especially cows and pigs. They are extensively used for food in the East and I am informed that at almost every meal in Japan and some parts of China the beans are served in some way. Some varieties are grown exclusively for the oil they contain and it is used for culinary, illuminating and lubricating purposes, the residue left after the oil has been expressed being used for fertilizer and also as food for animals. The light-colored beans are eaten in soups and the pods are sometimes picked green, boiled and served cold with a sprinkling of soy sauce. The green varieties are often pickled in brine and eaten moist or dried with meals as promoters of appetite; the same varieties are often slightly sprouted, scalded and served with meals in the winter time as a green vegetable. The bean forms the basis of the so-called soy sauces used as condiments all over the world. It is most frequently eaten in the more or less cheesy-like foods which are prepared from it. The most common of these are natto, tofu, miso, yuba and shoyu. Natto is a sort of bean cheese made by boiling the beans until they become very soft and then placing the resulting mass in a warm cellar, where it ferments. Tofu is prepared by soaking the beans for about twelve hours in water and then crushing them between mill-stones. They are then boiled in about three times the bulk of water and

filtered. The filtrate is a white, opaque, milky liquid with a taste similar to malt. This soy bean milk has a composition nearly the same as that of cow's milk:—

Water	92.53	per cent.
Protein	3.02	" "
Fat	2.13	" "
Fiber	0.03	" "
Ash	0.41	" "
Nitrogen free extract, including carbohydrate	1.88	" "

In making tofu the protein is precipitated by adding the mother liquid obtained in the manufacture of salt from sea water. The precipitate is filtered off and made into cakes. The large amount of nitrogen which these beans contain probably accounts for the small amount of meat and other animal food taken by the Japanese. According to the dietary studies made in Japan the nitrogen from the beans, especially if taken in a mixed dietary, is easily absorbed. The soy beans are sometimes roasted and then used as a substitute for coffee. They are also sometimes served as a vegetable, first soaking the beans until the skins come off and then boiling them until soft and serving hot.

The bean cheese is apparently not very well suited to the palates of people except those in the Orient. I do not think the beans will gain much favor as food stuff for man in America until further studies have been made in preparing them for the table. I have eaten them in a number of different ways and think that the taste for them would have to be acquired.

The North Carolina Experiment Station recommends the bean as a palatable vegetable and suggests that the beans be soaked until the skins come off and that they be stirred until the skins rise to the surface, when it is easy to remove them. The beans are then boiled with bacon until soft, seasoned with pepper, salt and butter and served hot. If the beans are green the preliminary soaking may be omitted.

The fact that the soy beans contain little or no starch suggested to Dujardin-Beaumetz that they be used for food for diabetics. The soy bean flour has been placed on the American market, but was withdrawn owing to the fact that according to the manufacturers it contained 8 per cent. carbohydrate. It con-

tains much less carbohydrate, however, than any of the other diabetic foods.

The soy bean meals have been analyzed by Winton and two specimens showed the following composition: One is said to contain 5.5 per cent. of starch and the other 7.63 per cent. Both specimens have essentially the same composition, showing 39.87 per cent. protein, 3.85 per cent. of fiber, 2.09 per cent. of nitrogen free extract, 19.06 per cent. of fat, 8.95 per cent. of starch, sugar and dextrin, which is determined by the diastase method without previous washing with water and is calculated as starch and 7.75 per cent. water.

There are variations in the composition of the different varieties of beans. The yellow beans grown in America have the following composition:—

Water	10.13	per cent.
Protein	34.63	" "
Fat	17.98	" "
Nitrogen free extract	30.50	" "
Fiber	3.69	" "
Ash	3.07	" "

Calculated to a water free basis:—

Protein	38.50	per cent.
Fat	20.	" "

As regards the use of the beans in infant feeding it seemed to me that soy bean gruel or milk, either alone or mixed with cow's milk, might be of value in feeding several classes of cases, viz., of marasmus and malnutrition, as a substitute for milk in diarrhea, and in intestinal and stomach disorders, and in diabetes mellitus.

There is, of course, but one way to determine the value of a new food and that is by careful experimenting. I had hoped to be able to make a more complete clinical report at this time, but several misfortunes have attended my efforts to secure the beans. My first crop was eaten by rats, my second moulded in the pods owing to some unusually damp weather, and insects ate about two-thirds of my last crop. Fortunately, the beans may now be obtained from Messrs. T. W. Wood & Son, Richmond, Va.

So far the gruel has been prepared by soaking the beans over night, stirring to remove the envelope surrounding the bean. Three times the amount of water is added to the beans and they

are boiled until a smooth gruel results. This is strained if necessary. This has the odor and taste of malt, but with the addition of a little salt is well taken, especially after the first bottle or two. The gruel is retained unusually well and seems to be easily digested. The stools are not more frequent than with other foods. The stools are light brown in color like those from malted milk. This soy bean gruel has nearly the same food value as milk and for certain children may need further dilution. About the same size feedings should be used as if milk were being given. Five per cent. sugar may be added to increase the fuel value.

I have not used the beans in a sufficiently large number of cases nor over sufficient periods of time to justify any further statements at this time, but I feel that properly used they will be a most valuable addition to the dietary of the sick infant. Grinding them to a bean meal would simplify matters very much, and, if success attends their use, a soy bean meal could easily be prepared.

I hope to be able to make a second report at the next meeting and have called your attention to the bean in the hope that other members may try them and report at the same time.

AN ABSTRACT OF FINKELSTEIN'S VIEW OF INFANTILE ALIMENTARY INTOXICATION.

BY IRVING M. SNOW, M.D.,
Clinical Professor of Diseases of Children, University of Buffalo,
Buffalo, N. Y.

Finkelstein, of Berlin, has recently published in the *Jahrbuch für Kinderheilkunde* a most interesting series of articles upon alimentary intoxications in infants. They have unfortunately attracted but little attention in the English-speaking world and the writer believes it would be of value to American physicians to have a condensed translation of Finkelstein's views. His ideas upon the etiology and also the cure of the gastrointestinal diseases of babies are those of the modern school of German pediatrists and are at variance with the accepted views of American authors. Finkelstein's theories are revolutionary, but are apparently clearly proven. He demonstrates his points by condensed histories of cases and by charts showing the reaction of weight, temperature and stools to various foods—breast milk, whole milk, skim milk, top milk, buttermilk and carbohydrate gruels. He believes that the digestive troubles of babies are more frequently due to perverted metabolism and to the toxic effect of food elements, fats and carbohydrates, than to bacterial infection. He calls the familiar infantile troubles intoxications and asserts he is able to show that not only faulty development, vomiting and diarrhea, but also more serious toxic signs—fever, cerebral disturbances, excitement and somnolence, choleraic and typhoid symptoms—may be due to diseased perverted processes of nutrition. The effects of food elements upon the digestion are studied as we watch the effect of medicines. He is able to produce symptoms and also to relieve them by increasing or reducing fats and carbohydrates. Casein, instead of being difficult of digestion, is important, easily assimilated and never causes serious symptoms.

Finkelstein is convinced of the unity of nutritional disturbances. Alimentary intoxication is the highest grade of a primary disturbance which progresses in an unbroken chain from health to the final catastrophe. All the different maladies, fermental diarrheas, ileocolitis, cholera infantum, acute and chronic dyspepsia and marasmus are only subdivisions of digestive disturbance of function, which, according to their intensity, produce

various symptom-complexes. One phase passes into another and all are caused to advance or recede by alimentary influence. Cases may present similar symptoms, but with the same treatment wide diversity is seen. With 3 children on the same food suffering from dyspepsia one is quickly cured, one recovers slowly, and a third falls into great danger from an acute attack of a choleraic diarrhea. A clear and vivid description of alimentary intoxication is given. The nervous symptoms are especially emphasized. The child may be crying and restless or may be somnolent, or even comatose. The respiratory changes are constant and often confusing. The breathing is quick and deep and audible. In severe cases it is panting, grunting and very rapid, simulating a pneumonia. The temperature may be insignificant, moderate or dangerously high. There may be dyspeptic stools, moderate diarrhea or violent vomiting, purging or loss in weight. A leukocytosis from infiltration of the intestinal walls from the toxemia, albuminuria and casts and an alimentary glycosuria are mentioned by Finkelstein as nearly constant symptoms. The sugar in the urine disappears with a water diet. In another type of intoxication the gastrointestinal symptoms are mild, but the patient is apathetic or even unconscious. Too much attention is usually paid to the gastrointestinal symptoms and too little to the constitutional and nervous disturbances. The local digestive symptoms are often no index to the danger. The most severe intoxications may appear with a slight dyspepsia or normal stools. The deep, rapid breathing, apathy or restlessness and glycosuria are the most trustworthy signs of intoxication. Regarding food injuries 3 illustrative cases are cited. Two children who invariably reacted on cow's milk with fever and bloody, mucous stools, and a third who sickened on malt soup. In all 3 the signs of intoxication quickly disappeared on a water diet. There was a close association between the giving of food and intoxication symptoms. If we investigate what changes in the nourishment seem to lead to the development of an intoxication, we find that there is a relationship between the quantity as well as the kind of food. In the presence of a disturbed digestion a change for the worse will occur if the quantity of food is suddenly increased. Finkelstein, after numerous experiments in his institution, succeeded in absolutely preventing the occurrence of intoxication. It was possible to avoid the impending danger by lowering the amount of food. He was also able, by increasing the food, to

experimentally produce an intoxication. *Example.*—With 2 children in the preliminary stages of an intoxication the food was increased and a catastrophe followed. The toxic symptoms were quickly relieved by the complete withdrawal of food and the administration of water and unsweetened tea. In one the cure was complete by crisis in twenty-four hours. With the other the detoxication was less rapid, yet it surely followed with a fall in temperature. In the case of incurably sick children the effect of starvation is "nil." Yet these cases could be proved to belong to the same pathologic group as the 2 more benign types. After complete detoxication a relapse may occur from an unwise increase of food. The nourishment should be increased gram by gram, with careful observation of weight, temperature and number of stools. In severe cases he gave tea for days with 5 c.c. of food from five to ten times daily. The food is not increased until the urine is free from sugar. The increase or diminution of food must be adjusted to each case, according to its severity. In slight intoxications a fair amount may be given. In fully developed intoxications or in atrophic children very small quantities should be used. The effective dose is always under physiologic need.

RELATIONS BETWEEN THE INTOXICATIONS AND THE KIND OF FOOD.

The effect of food elements can be tested by adding or subtracting them from the diet and observing the reaction. The result of the additional increase of sugar of any kind is to precipitate the outbreak of the intoxication and its diminution or withdrawal is to cause a disappearance of symptoms. Foods rich in sugars, especially the much lauded buttermilk, are dangerous. The number of intoxications in Finkelstein's institution has steadily sunk with the careful use of sugar and a relapse in convalescence is usually attributed to too much sugar. Detoxication is delayed or prevented when much sugar is added to the water. Proprietary foods may cause intoxication in proportion to their amount of soluble carbohydrates. Thin cereal gruels have no pathologic effect. Fat is a food element whose ingestion has a striking effect on intoxication symptoms. Numerous personal experiments show that if the existing tolerance of fats is exceeded, mild disturbances develop into complete intoxication. If the child be already sick it is made worse or a relapse is excited. Even slight quantities of fat may cause serious symptoms. The author states

that during an intoxication the toxic quality of fat depends on the patient's susceptibility to fat and not to the intestinal flora. With children whose intoxication is due to severe milk injury fat is especially toxic. It is of great practical importance to foresee the effects of human milk in a sick child. Wiederhofer states that many cases of cholera infantum will not stand breast milk. Salge has shown how common a fat injury is in intestinal catarrh. Breast milk is injurious from its fat. If fat free it will be tolerated. If an intoxicated baby takes large quantities of breast milk the cure is much slower than if put on a water diet. In a second phase of disease the breast milk first increases the trouble, then follows a slow improvement. In a third class breast milk may hasten a fatal termination. Centrifugal breast milk can be given in large quantities with good effect except in almost total destruction of the digestive functions. Its curative properties reside in part free of fat and sugar. The opinion is erroneous that an intoxicated baby that cannot tolerate breast milk will die. Most of these children will improve if given a day on water and then small quantities of food. This method will cause the most astonishing cures in desperate cases. In contrast to sugar, fat increases the intoxication only when the process is active. At other times fat may be injurious, but not in the way of causing an intoxication. With children fed on diluted, whole or top milk mixtures with little sugar the severe disturbances are of the nature of atrophy and not intoxication.

RELATION OF SKIM MILK TO INTOXICATION.

If skim milk is given relief does not follow as quickly as with water. Fever should always be allowed to subside before skim milk is given. Skim milk, however, will seldom make a child worse. Skim milk consists of casein and whey. Experiments show that casein can be given mixed with water during an intoxication with as rapid relief as with water. Albumin is equally harmless. Whey, on account of its sugar, is a bad substitute for casein and has about the same effect as skim milk.

DEPENDENCE OF THE SYMPTOMS UPON THE CONDITION OF THE CHILD.

One child recovering from an intoxication will tolerate a food while the same thing makes another worse. The severity and the acuteness of the intoxication symptoms give no conclusion as

to the severity of the blow to the organism. It is surprising how one case will resist therapy while another with the most violent symptoms will rapidly improve. The explanation of this variation is in functional tolerance. The nearer the child is to atrophy the more unfortunate is the occurrence of an intoxication. Up to a certain point we can control the appearance and disappearance of intoxication by dietetics. Continual progress of the original injury is in great part due to faulty diet. In times past many of his patients have suffered from lack of judgment in feeding. Possessed by the idea of the prevailing teaching that severe and feverish disease was due to bacteria and that the case needed abundant nourishment to offset the effects of the infection, Finkelstein allowed children to take food at will, and did not understand that the withdrawal of food and limitation of food constituents was the only way to save. He thinks that his sad experience with chronic, feverish and diarrheal conditions was due to alimentary intoxicants and should have been cut short by correct dietetics. These conditions should be properly studied by a chart with curves of temperature, weight and stools, as influenced by the kind of food.

Apparently accidental changes are the direct result of feeding. The clear association between quantity and kind of food and the rise and fall of the intoxication make it impossible to retain the idea that acute, feverish, toxic aggravations of a disturbed nutrition are the result of an accidental infection. If infections are the cause, how is it, during a whole year with improved dietetics, intoxications in nutritional disorders have been completely suppressed? With certain diets no intoxications occur; with others they are very common. As to the question whether spoiled milk causes the trouble, if one remembers that woman's milk will cause the most severe symptoms, and that our observations have been made in an institution where the nourishment is absolutely free from suspicion, and that of numerous infants, all fed alike, only one child would be intoxicated, the belief will disappear that infected milk will cause intoxication. Intoxications of alimentary origin will appear when a sick child is given larger quantities of food or of certain food elements than its already injured functions can tolerate. Contrary to the prevailing teaching, casein does *not* cause intoxication. Fat and sugar are the causes of intoxication. Detoxication is only possible by excluding fat and sugar. The malady yields as slowly on whey as on skim milk, and the cases

improve on casein and water as rapidly as on water alone. The study of appropriate cases shows that the toxic effect of fat is seen only in cases where the organism is badly damaged in a specific way from other causes, usually by a sugar injury. The laxative effect of sugar is well known. From now on it must be counted as the cause of severe and often fatal toxemia. This dangerous property exists in all kinds of sugar. The difference in various sugars requires further study. The effects of mineral salts in milk are not known. It may be urged as an objection to the theory of the alimentary nature of the symptoms of intoxication, that some cases are not benefited by the withdrawal of food. There is a stage in functional injury when even without the addition of more toxic material to the body it is incapable of detoxication or excretion of the existing poisons. The child dies before improvement can occur. An infectious complication may also exist, which may cause fever and hinder a recovery. The poison of the intoxication does not come from bacterial decomposition of the intestinal contents, but from disturbed metabolism. From this standpoint it is conceivable that in the most severe cases, not only the intestines are injured by the food elements, but also the body reserves of fat and glycogen are affected, and that the alimentary intoxication is a true autointoxication. Alimentary intoxication has been compared to an acid intoxication; however, starvation, which causes an acidosis, relieves intoxication. Fat and sugar will cause symptoms of intoxication. The malady is caused by certain food elements, which are given in greater quantities than can be consumed by the diseased organism. The idea should be emphasized that food may have poisonous effects without bacterial infections.

There is an alimentary fever, and the teaching of the older physicians as to the heating effects of food and the starvation diet of fevers rests on facts. Food becomes a poison only when the metabolism is severely injured. The intoxication is only a transitory symptom, which brings the existing malady to light.

Czerny and Keller have made an important advance by the creation of the term "food injury." One should accustom themselves to associate food with a specific functional stimulus and to consider processes of nutrition as complicated biological reaction which are started through the stimulus of the ingested food and which, after passing through a number of phases, reach their end by the construction of tissue and by the expulsion of

excreta. The clinician should closely observe the effects of food upon the functions of the body. The reaction of food is as prompt as a reaction to poison. To distinguish alimentary from non-alimentary symptoms the effect of food changes in food elements or of the withdrawal or increase of nourishment should be noticed. All symptoms which are influenced by change in diet are to be regarded as originating in nutritional processes. In health all functions are performed in a quiet, orderly way. The healthy child possesses a strong immunity and is especially characterized by a physiologic breadth of tolerance for various kinds of food. Provided the food fills the nutritional needs the digestion is equally good with breast or bottle, concentrated or diluted nourishment with more or less fats or carbohydrates. It will respond to a moderate increase in food by a gain in weight. If a baby thrives on the usual artificial diet of milk and sweetened water it is the best security for continued health and development. With a disturbance of nutrition there is a lowering of tolerance for both quantity and kind of food. The strength of digestion should be tested by giving small quantities of food. In intoxication no dietetic test can be used. After detoxication the degree of injury is evident when nourishment is given. Symptoms will recur when the function of assimilation is exceeded, and excepting during a terminal attack there is always a diet which can be digested with ease.

Finkelstein gives four types of disturbance of nutrition with numerous transitional stages.

First Stage.—Disturbance of balance; the breadth of tolerance is lessened; with a limited diet the child is normal. An increase or variation of food will diminish the weight.

Second Stage.—Stage of dyspepsia. The degree of tolerance is much diminished, so that the child only remains free from symptoms when the diet is just at, or under, the need of nutrition. If this limit of food be exceeded a paradoxical reaction occurs with pathological stools and variations of temperature.

Third Stage.—First severe type decomposition or atrophy. The toleration of food, first against fats, later against sugar, is injured in the most extreme degree. With an increase of food the child reacts with a sudden loss in weight, subnormal temperature, irregularities in the rhythm of breathing and cardiac depression.

Fourth Stage.—Intoxication. Already described.

There are two severe forms of infantile nutritional disturbance which may exist separately or may be associated. First, alimentary intoxication. Second, alimentary decomposition or atrophy. A condition which is free from all signs of toxemia and which passes through the stages of disturbance of balance and afebrile dyspepsia with continual emaciation to a state which I will designate as alimentary decomposition.

Finkelstein's observations were made only on children who were given milk mixtures. In the stage of balance disturbance the organism assimilates the foods in necessary quantities, but does not do the work with normal steadiness. The changes in the stools, fat or soap stools, are discovered only by a laboratory analysis. There are slight fluctuations in temperature, irregular gains or losses in weight. We find a characteristic paradoxical reaction with food. Only a limited amount is utilized. If the food be increased there is more work but no gain. With food elements whose utilization offers special difficulties, the weight is diminished. Regarding the doctrine of the difficult digestion of casein no change was noticed from its increase or diminution, excepting from a physiologic lack of albumin. There was no difference between whole milk, skim milk or buttermilk. The same successes, failures and consistencies of stools. Sweetened buttermilk has the same effect as skim milk with a carbohydrate. With a disturbance of balance the most important rôle is played by fat. It is not possible by increasing the fats to hasten the gain in weight; further, many children do well on fat poor or fat free carbohydrates and the gain in weight is arrested when fat is added to the food and commences again when fat is left out. This is a paradoxical symptom when the exclusion of what is usually held to be a food element of the first class has a favorable effect upon the weight. Children who digest fat poorly usually possess an undiminished tolerance for carbohydrates, and often make great progress on fat poor carbohydrate rich diet—the malt soups, buttermilk, etc.

This experience has been utilized for years. Assimilation of fats makes greater requirements upon metabolism than carbohydrates. In patients who do not progress satisfactorily on a fat free diet, normal relations are only preserved when carbohydrates are closely restricted. A gain is not possible on this limited food. If one tries to raise the carbohydrates dyspeptic symptoms appear. Only with older, healthy children, with a fat free, carbohydrate

diet, will one see a stationary weight without dyspepsia. This is practically a disturbance of balance and the addition of fat has a favorable effect upon the weight. After a certain abstinence fat is necessary. Mother's milk is beneficial in all cases of disturbance of balance. It quickly improves nutrition and weight. The exception is children with a congenitally weak metabolism.

STAGE OF DYSPEPSIA.

A baby with balanced disturbance passes easily into a dyspeptic condition. The cause may be contaminated milk, but is more frequently a fermentation produced by a disproportion between the quantity of food and the strength of the digestion. The clinical picture of dyspepsia is purely local, abnormal fermentation with its reaction upon the motor and secretory functions of the gastrointestinal tracts. Only when the amount of food is far under the physiologic necessity is the child free from symptoms. With an increase in food beyond the digestive tolerance there are frequent pathologic stools, fever and emaciation. A severe injury of the intermediate metabolism places the condition outside the boundaries of dyspepsia and indicates the approach of danger, intoxication or atrophy. Atrophy or alimentary decomposition may simulate dyspepsia when the nourishment is much restricted. The influence of correct dietetics in the dyspeptic troubles of bottle-fed babies is shown in the curative effect of woman's milk. The normal activity of the intestines returns and the stools and flora become those of a breast-fed infant. Fat is an element of cow's milk which causes fermentation. The exclusion of fat will often relieve dyspeptic vomiting. An excess of carbohydrates will also produce fermentation. Many dyspeptic troubles are stopped by cutting out carbohydrates and diluting the milk with water. Starch dyspepsia is well known. The tolerance for malt or dextrine is often greater than for milk or cane sugar. It is a common experience in dyspepsia to obtain improvement by Liebig's soup or malt extract. The best remedy for dyspepsia is skim milk or buttermilk. With correct indications and dose this fat free sugar poor diet will cure every true dyspepsia. The lactic acid bacillus is not the curative agent in buttermilk. Cooked buttermilk or skim milk are equally effective. Clean, fresh skim milk from the separator should be used. Its therapeutic value is due to the absence of fermentative elements. In many cases intestinal digestion is so weakened that the slight

amount of sugar in skim milk will cause disturbance. Cases of dyspepsia which, in spite of restricted diet, are not considerably improved at the end of a week are in the first stages of atrophy and can usually be saved only by the breast.

As to the apparent failures of skim milk therapy slight enteral and parenteral infections and influenza may appear in the guise of dyspepsia and are not favorably influenced by diet. Slight cases of dyspepsia react to large amounts of food and are relieved by slight diminutions. Severe cases require so scanty a diet that the weight cannot be maintained. Protracted dyspepsia weakens the resistance of the organism to chemical and bacteriologic damage. There is increasing injury to the functions which dispose of the food in normal metabolism. With the destruction of these external bulwarks there begins a disturbance which will end with death if not prevented by timely medical aid.

Dyspepsia is a borderland state leading to intoxication or atrophy.

ATROPHY OR ALIMENTARY DECOMPOSITION.

Atrophy is best studied in children fed on top milk or milk diluted with cereal gruels. At first there are slight gains and losses in weight. With a transition from dyspepsia to atrophy there is a steady emaciation. The child becomes excitable, has disturbed sleep and takes the bottle greedily. The stools may be at first normal, especially if the baby is breast-fed. Later we observe soap or fat stools. The wasting may be slow or rapid, like a death from starvation with abundant nourishment. The more food given the thinner the patient becomes. The only way to arrest the loss in weight, if, indeed, it be not too late, is to diminish the food. An excess of food beyond the child's small tolerance destroys the tissues which in healthier days are constructed. Often an atrophic child is improved by minute quantities of breast milk, while a theoretically sufficient amount has a disastrous effect. Usually increasing the fats in any way will reduce the weight. Emaciation may follow this change from a salt rich to a salt poor diet.

A stationary weight does not mean the nutritional needs are covered. Even with insufficient calories, if enough salt and water are given, the loss in weight is not seen for a long time. In atrophy the food elements which cause the wasting are fats and carbohydrates. A case will often improve if skim milk be sub-

stituted for whole milk. The tolerance for fats is first injured, later the tolerance for carbohydrates. An atrophic child on a fat poor mixture grows rapidly worse if cream is added. Increasing the fats accelerates the loss in weight and may cause a fatal collapse. Wonderful as it may seem, disturbances of heart beat and rhythm of respiration may be caused by addition of fats and relieved by their withdrawal. In severely disturbed children fat acts as a specific poison to the heart and respiration. Fat, esteemed as the best heat producer, will, in an atrophic child, cause a lowering of temperature. The same toxic effects are noticed after castor- and cod-liver oil.

While the assimilation of fats is destroyed the tolerance for carbohydrates may be preserved for a while and on them a good increase in weight may be maintained. But only a few children react in this favorable way. Many children show at once a lowered carbohydrate tolerance and the elimination of fats and carbohydrates has usually a beneficial effect in atrophy. In advanced cases the weakened functions will not grow on skim milk. In fact, the patient cannot be nourished at all, and only in the earlier stages of atrophy can its course be stopped by diminishing the food. It is peculiar that the functional tolerance starts from a certain moment and steadily progresses in spite of reduction in quantities of food or changes in food elements.

Finkelstein divides atrophy or alimentary decomposition into three stages:—

(1) Resembles dyspepsia. Restriction in food is followed by an arrest of emaciation and improvement in stools.

(2) Loss in weight is arrested very slowly. Stools remain pathologic. Increasing the food causes a recurrence of symptoms. Recovery by artificial food is exceptional.

(3) The functional tolerance is almost gone. It is not to be controlled by even small amounts of food. Its weakness is demonstrated by the injurious effects of rich breast milk.

In most cases death is due to a terminal intoxication from sugar. The steady fall of the weight curve measures the declining tolerance for food. Finkelstein's experience with artificial feeding in atrophy is that if the functional tolerance has not been augmented in a few days a further trial of the bottle is useless. To continue it is a choice between death from decomposition and death from hunger.

REACTION OF AN ATROPHIC CHILD TO BREAST MILK.

One occasionally sees the typical picture of marasmus in a breast-fed baby. There is a stationary period for weeks, then a downward curve. Assimilation of fats is badly damaged and the case recovers only on a carbohydrate diet. So even with woman's milk the evil consequences of a lowered fat and sugar tolerance cannot be avoided. The wonderful therapeutic power of breast milk probably resides in the portion free of fat and sugar. If breast milk be given to an atrophic child only minute doses should be administered at first. In bad cases 40 or 50 grams daily, later 100 to 150 c.c. The need for fluids is to be met with water or tea sweetened with saccharine. The danger of inanition must be risked. In this way a few children can be saved by a special technique. Finally, atrophy is the progressive lessening for all foods. It is alimentary and is not a simple emaciation from starvation. All the tissues of the body are involved. There is a dangerous fall in the strength of digestion reaching complete destruction of function. The full energy of the digestive powers is necessary for life. Their weakening or inactivity leads to death. Food, after overcoming the primary resistance of the child, increases the trouble, hence the creation of the term alimentary decomposition.

BIBLIOGRAPHY.

Jahrbuch für Kinderheilkunde. Über alimentäre Intoxications im Säuglingsalter von Prof. Dr. H. Finkelstein im Berlin.—Januar, 1907, p. 1; März, 1907, p. 263; Juli, 1907, p. 1; November, 1908, p. 521; Dezember, 1908, p. 692.

INVESTIGATIONS UPON THE IRRITATING EFFECTS OF CATHARTIC DRUGS.

BY I. A. ABT, M.D.,
Chicago.

This inquiry suggested itself because of the common practice of giving infants and young children a cathartic drug for a more or less continuous period irrespective of the indications for its use. The conviction has grown upon me that the clinical course of some of the disorders of infancy is protracted by the excessive or untimely use of these drugs. I think there are many in a position to corroborate my experience with children who, taking calomel for a supposed gastrointestinal disorder and suffering coincidently from severe colic, mucous and diarrheal stools together with loss of weight, improve when the cathartic drug is discontinued and a rational diet encouraged. It would be absurd to decry entirely the use of laxatives in infantile therapeutics; they have their distinct indications. By a series of observations which I have made, I hope to show that this group of drugs has its therapeutic limitations and that the so-called cleaning out process should not be continued after catharsis has been established. I am convinced that such a process in itself frequently enough produces irritation. It is self-evident that after an initial dose of laxative has acted and the toxic food stuffs have been evacuated, the indication is not for more laxatives, but, first and foremost, for rest for the bowels, and, secondly, for a bland, unirritating diet which requires little or no digestion and renders putrefaction and fermentation next to impossible.

THE OBSERVATIONS.

Cathartic drugs were administered about 125 times to 22 children in the Michael Reese Hospital, Chicago, to determine, if possible, the irritating effects of average doses of the commonly employed cathartics, such as calomel, castor-oil and Epsom salts. The stools were examined before the administration of the cathartic was begun. After the cathartic had been administered in varying doses for twenty-four hours the stool was ex-

amined in the usual manner for color, odor, consistency, reaction, microscopic findings and degree of oxidation of the bile pigments. Particular stress was laid upon the presence of blood and mucus in the feces, these elements being taken as an index of the degree of irritation. In the report of cases which follows, little emphasis is put upon other findings noted in the course of the routine examination. The report would be too long drawn out and of not enough interest to give the minute details of every case.

CASE I. M. C. Wet-nurse's child. Aged three months. Normal stools before administration of cathartic. One grain of calomel, in divided doses of ¼ grain each, given on two successive days.

Second day. Stool contained free mucus; blood positive by both benzidene and Weber tests.

Third day. Mucus more finely divided; benzidene reaction strong.

Sixth day. One grain of calomel again given.

Seventh day. Much finely divided mucus; benzidene test strongly positive.

Eighth day. Mucus had nearly disappeared; benzidene test very faint.

CASE II. E. S. Malnutrition baby. Aged five months; fed on predigested milk. Stools normal before giving cathartic. One grain of calomel in $^1/_{10}$ grain doses caused no signs of irritation except a small amount of finely divided mucus. Four days later 2 grains of calomel were given in divided doses of ¼ grain each.

First day after, stool was offensive; contained much mucus; benzidene test positive.

Second day after, mucus less abundant; only a trace of blood.

Third day after, more blood than before; Weber and benzidene tests both positive.

CASE III. B. B. Mongolian idiot. Eight months old. Was given $^1/_{10}$ grain of calomel three or four times daily for nine days.

Fourth day. Stool contained no mucus; small trace of blood by benzidene test.

Eighth day. Small quantity of mucus in short shreds; trace of blood by benzidene test.

Ninth day. Mucus abundant and in free shreds; Weber and benzidene tests for blood distinctly positive.

CASE IV. Bessie L. Infant. Stools were abnormal before beginning calomel; contained mucus; $^4/_{10}$ grain of calomel was given without causing any additional signs of irritation. The offensive odor was lost.

CASE V. T. J. Six years old; syphilitic child. Was given calomel for twenty-four days. First two days $^1/_{10}$ grain four times daily. After this $^1/_{10}$ grain six times daily.

First day. No signs of intestinal irritation.

Second day. Small quantity of mucus in short strings.

Third day. A trace of blood by the benzidene test.

Fifth day. Free mucus much increased; positive benzidene test.

Seventh day. Stool contained much more mucus and blood; benzidene test reacted strongly, Weber faintly.

Tenth day. Much more blood; both Weber and benzidene tests reacted strongly.

Tenth to twentieth day. Mucus and blood present in greater or smaller amounts; occasionally one or both were absent.

Twentieth day. Mucus in large quantities and blood positive by benzidene and Weber tests.

Mucus and blood were found in smaller quantities until the twenty-fifth day, when calomel was stopped.

CASE VI. Baby B. Stools abnormal before calomel was commenced; contained free mucus. One grain of calomel given.

First three days after calomel, stools contained considerable mucus, more finely divided than previous, but no blood.

Fifth day. Three grains of calomel given.

Sixth day. Stool contained much finely divided mucus; decided reaction by benzidene test; Weber test negative.

Seventh day. Mucus more stringy; blood reaction less marked.

CASE VII. Jonas S. Five years old. Generalized eczema. Stool normal before administering calomel. One grain of calomel given on three successive nights followed by 2 drams of magnesium sulphate on the morning of the fourth day.

Second day. Stool contained small quantity of finely divided mucus; trace of blood by benzidene test.

Third day. Free mucus in large shreds; benzidene test markedly positive.

Fourth day. Day after 3 grains had been given; free mucus

in strings and abundant; Weber and benzidene tests strongly positive.

Fifth day. Mucus had diminished slightly; reaction for blood with benzidene test only.

Ninth, tenth and eleventh days. One grain of calomel given each evening.

Twelfth day. Magnesium sulphate, 2 drams of saturated solution given in the morning.

Tenth day. No mucus visible microscopically; benzidene test faintly positive.

Eleventh day. Much free mucus; benzidene test strongly positive.

Twelfth day. The day after the last grain was given; stool watery; solid portion almost entirely composed of mucus; reaction for blood strongly positive by Weber and benzidene tests.

Thirteenth day. Mucus still present in quantities; both blood tests more pronounced.

Fourteenth day. Mucus less; reaction for blood less pronounced.

Fifteenth day. Weber test for blood negative; benzidene test faintly positive; mucus nearly disappeared; was finely divided.

CASE VIII. B. D. Eighteen months old. Three and one-half grains calomel in three days.

Fourth day. Stool formed, accompanied by discharge of much brownish fluid, with considerable free stringy mucus; reaction for blood positive with benzidene test; negative with Weber test.

CASE IX. S. S. Nine months old. Malnutrition infant. Fed on predigested milk; stools normal before administration of cathartic. Was given 1 dram of saturated solution of magnesium sulphate for three successive days.

Second day. Stool contained neither blood nor mucus.

Third day. Trace of blood by benzidene test; no mucus.

Fourth day. Benzidene test reaction for blood markedly positive; no mucus seen macroscopically.

Fifth day. Stool seemed normal, except for a faint benzidene reaction.

CASE X. L. S. Ten months old. A malnutrition baby. Stools normal before beginning test. One dram of saturated solution of magnesium sulphate was given for three successive days.

Second day. Stool soft; contained mucous shreds; no blood.

Third day. Stool semi-solid; much free mucus, and a trace of blood by benzidene test.

Fourth day. Mucus less abundant; benzidene reaction more sharply marked; Weber test negative.

CASE XI. John B. Nine months old. Malnutrition infant. Stools normal before beginning observation. One dram saturated solution of magnesium sulphate for three successive days.

Second and third days. Stools unchanged; contained neither blood nor mucus.

Fourth day. No mucus visible macroscopically; benzidene reaction for blood positive; Weber test negative.

CASE XII. Mary W. Healthy baby eight months old. Breast-fed, normal stool. One dram of saturated solution of magnesium sulphate given.

Second and third days. Small quantity of free mucus; no blood by either test.

CASE XIII. Gus. J. Four months old. Healthy infant; breast-fed. Stools normal before administering cathartic. One dram saturated solution of magnesium sulphate given.

Second and third days. Stool normal; contained neither blood nor mucus.

CASE XIV. Pearl B. Nine months old. Stools normal. Was given 1 dram of castor-oil.

Second and third days. No mucus present; reaction for blood negative.

CASE XV. L. R. Eleven months old. Normal baby; normal stools. One dram of castor-oil given.

Second and third days. No mucus present; reaction for blood negative by both tests.

CASE XVI. George S. Two years old. Malnutrition baby. Two drams of castor-oil given.

Second and third days. Stool contained neither blood nor mucus.

CASE XVII. Gerald S. Chorea. Five years old. Stool watery and offensive before administering cathartic. Three drams of castor-oil given at one dose.

Second and third days. Stool has improved, odor less offensive, better consistency; no blood or mucus.

CASE XVIII. M. M. Six months old. Normal stool. One dram of castor-oil given.

Second and third days. Small quantity finely divided mucus; trace of blood by benzidene test.

CASE XIX. S. D. One year old. Bronchopneumonia. Stool undigested and contained much stringy mucus. One dram of castor-oil given.

Second and third days. Condition of stools improved; mucus less abundant and more finely divided; no blood present.

CASE XX. H. E. Sixteen months old. Cervical adenitis and lobar pneumonia. Stools contained mucus before beginning cathartic. Two drams of castor-oil given.

First day. Four hours after giving oil much stringy mucus; benzidene reaction faintly positive.

Second day. Free mucus abundant; strongly positive benzidene test for blood.

CASE XXI. M. W. Eight months old. Healthy infant; normal stool. One dram of castor-oil for three successive nights.

Second and third days. Stools unformed; contained neither blood nor mucus.

Fourth day. Small quantity of finely divided mucus; faint trace of blood by the benzidene test.

CASE XXII. S. S. Ten months old. Malnutrition infant. Stool before beginning observations clay-colored, but contained no blood or mucus. One dram of castor-oil given.

Second day. Stools unformed, otherwise unchanged.

Third day. Stools unformed, otherwise unchanged.

Fourth day. Small amount of free mucus; positive benzidene test for blood; Weber test negative.

OTHER OBSERVATIONS ON EXAMINATION OF FECES.

As has already been stated, the observations which follow are not given in detail, but are mentioned herewith in condensed form.

(1) *Color.*—In most cases unaffected. Green stools especially after calomel. Those which were at first yellow occasionally became dark brown or mottled. Several observations show that stools of abnormal color became normal in color with any of the cathartics used.

(2) *Odor.*—The most frequent observation in reference to

the odor was that an offensive stool became less offensive. Occasionally it occurred that the stool became more offensive after the use of the cathartic, and this was particularly true of the calomel.

(3) *Consistency.*—In consistency the stools were rendered, for the most part, softer, but seldom watery.

(4) *Reaction.*—In regard to the chemical reaction of the stools, the majority were faintly acid before the cathartics were given and were unaffected by the laxatives. Occasionally an alkaline stool was met with.

(5) The microscopic findings were, for the most part, constant. No blood corpuscles were found in any stool. After catharsis had been established, epithelial cells were increased and were found imbedded in masses of mucus. They were frequently bile stained.

(6) *Bile Pigments.*—Unoxidized bile pigment was found in many cases before and after the cathartic was given. The quantity was usually larger after catharsis. Calomel seemed to produce the most marked effects on oxidized bile pigment when present.

(7) The effect of calomel upon the bacterial contents was determined in 1 case. Four days after 1 grain of calomel was given the bacteria numbered 210,400,000 bacteria per gram of feces. On the next day, after 3 grains of calomel the count rose to 762,300,000—decreasing two days later to 170,000,000, increasing on the following day to 276,300,000.

RÉSUMÉ.

(1) Calomel was given to 8 children, 60 stools being examined.

(2) The children who received 1 grain of calomel, either singly or in divided doses, showed irritative effects on the following day, as evidenced by the presence of mucus and occasionally of blood.

(3) Those who received 2 or 3 grains of calomel at one dose showed both blood and mucus the following day; this finding persisted for two to three days after the discontinuance of the cathartic.

(4) In cases where calomel was given in daily divided doses ($^1/_{10}$ grain per dose) for a consecutive number of days, the reaction for mucus and blood increased in intensity from day to day.

(5) Magnesium sulphate was given to 5 children; 18 stools were examined. Small single doses, as 1 dram of the saturated solution produced slight irritation, *i.e.*, small quantity of mucus, but no blood.

(6) Large doses of magnesium sulphate (1 dram of saturated solution for three successive days) showed at the conclusion of the third dose a reaction for blood by the benzidene test and considerable mucus, though not as much as was produced by repeated doses of calomel.

(7) Castor-oil was given to 9 children, 26 stools being examined. Single dram doses of castor-oil produced no irritation, except a slight reaction in 1 case. Two drams given at one dose to older children caused the reaction of irritation in 1 case; another case showed no irritation in the stools.

(8) Castor-oil was given to 2 children in dram doses for three successive nights. Both stools showed the reactions of irritation after the third dose.

(9) All three cathartics under observation are capable of producing signs of irritation if the dose be sufficiently large or given for a continuous period of time. Of the three drugs under observation calomel seemed capable of producing the greatest irritation with average doses in the shortest possible time. Castor-oil seemed to cause the least reaction.

I am indebted to Dr. Mark Jampolis, of Chicago, for his painstaking and diligent labor in carrying out the analyses enumerated above, and also to Dr. Con. Jacobsen for helpful suggestions and chemical and bacteriologic reports of specimens submitted.

BLOOD TESTS.

A piece of feces about the size of a large lima bean is mixed with a small amount of water. The fat is then extracted by shaking with a small amount of ether, which is poured off. About 2 c.c. of glacial acetic acid and 5 c.c. of ether are then added to the stool and the mixture again shaken. After settling, the acid-ether extract is poured off and used for the Weber and benzidene tests.

Weber Test.—To one-half this mixture about 15 drops of a fresh tincture of guaiac and about 2 c.c. of hydrogen peroxide are added. In the presence of blood the mixture turns blue, the intensity of the color varying with the amount of blood.

Benzidene Test.—(Test of O. and R. Adler.) The benzidene test was performed in the same way, 15 drops of a solution of Merck's benzidene in acetic acid being added instead of the tincture of guaiac. As in the Weber test the color in blue varies in intensity with the amount of blood. Benzidene test is about eight times as sensitive as the guaiac test.

(Described in *Zeitsch. für Phys. Chem.*, 1904.)

DISCUSSION.

DR. MORSE.—I am glad to have Dr. Abt prove by observation what I have been coming to believe for some time, that calomel is a comparatively dangerous drug and that castor-oil is much safer. The average physician is, I think, in the habit of giving too many cathartics in disturbances of the intestinal tract.

DR. SOUTHWORTH.—I would like to say a word in favor of calomel. We have been in the habit at the out-patient department of the Babies' Hospital of dispensing broken doses of calomel in tablet form. I have used a great many thousands, probably tens of thousands, and I have never been convinced that any harm was done by it when employed rationally.

DR. ADAMS.—I believe in giving calomel, but I believe a great deal of confusion arises from the fact that the doctor often gives calomel before making the diagnosis. The first thing the average practitioner does on seeing a case is to write a prescription for calomel. I made that statement at a meeting of physicians when an enthusiastic young physician got up to squelch me and said such a charge was unfair to a great profession and was not true. I said, "There are thirty-five men here; will one who doesn't do this hold up his hand?" and not one held up his hand. The diagnosis is often confused by such a procedure.

DR. BLACKADER.—This subject of cathartics is of interest to all of us. In these days of therapeutic nihilism, purgatives form the most frequently employed class of drugs; many who have thrown overboard almost all other classes of drugs still employ members of this group in a more or less modified way. The facts brought out by this paper of Dr. Abt's are, therefore, of great interest to all. The doctor has chosen the three purgatives which are regarded as the least irritating. In his employment of them my only criticism is that the dose of magnesium sulphate was a drastic one for an infant. Pharmacologists recognize that all cathartics are more or less irritating to the mucous membranes of the intestinal tract, inducing increased secretion and increased peristalsis, with a lessened capacity for absorption. I do not

think the question of how much irritation is produced by our cathartics has ever before been investigated by examination of the feces. A slight increase in the amount of mucus, with the appearance of a few red blood cells, may or may not be of importance. It would be interesting to investigate the action of our other cathartics and perhaps to inquire whether all conditions which induce an irritative action in the bowel are not accompanied by the appearance of red cells.

The paper is a very valuable one and a very timely one.

DR. ROTCH.—I have always made it a practice to give very little calomel. I think it is given unnecessarily many times. In comparing my cases with those of others who give calomel I cannot see but that my cases do just about as well. It is very seldom that I give calomel, especially in the early years. I rely much more upon the management of the food.

DR. ABT (closing remarks).—I may say that I was led to carry out these observations, because I was impressed with the fact that these drugs were abused and over-used in practice. I am now convinced that small doses of non-irritating cathartics will act quite as well, or better, than long continued or excessive doses of calomel.

As to Dr. Blackader's remark about dram doses of magnesium sulphate being large for a child, I should say that I used dram doses of saturated solution of Epsom salts.

A CASE OF CEREBRAL HEMORRHAGE (BIRTH), WITH OPERATION.

BY FRANK S. MEARA, M.D.,

AND

ALFRED S. TAYLOR, M.D.,

New York.

Of the unrecognized causes of death at birth, cerebral hemorrhage undoubtedly affords a considerable percentage. The cases of hemorrhage surviving the act of parturition offer a symptom-complex readily appreciated by the obstetrical specialist and less and less overlooked by the general practitioner functionating as accoucheur. As tradition and usage have made the obstetrician a pediatrist for four weeks, those of us who affect the latter title as a working *nom de plume* see only the pathetic picture of psychical and physical impairment and blasted parental hopes.

It is not my purpose to "carry coals to Newcastle" in outlining a familiar picture of either the early or late manifestations of this accident or invite maledictions for again spinning out a many times reiterated bibliography, but I wish, with the emphasis always guaranteed an individual experience, to call your attention to the only measure that has been followed by any degree of success in such cases, namely, operative interference, and to show where, in case of failure, the cause of failure lay.

On February 17th, late in the afternoon, Mrs. M. B. went into labor. She was thirty-five years old. The present was her first pregnancy and it had been uneventful. The pelvic measurements were liberal and promised no obstruction. Her labor was very slow and the first stage was not completed at the end of forty-six hours. During this time the uterus seemed to have no driving force at all and the only result of the pains was increasing fatigue. I decided to force the labor at once. The cervix was at that time three fingers dilated, having made no advance for some time. Champétier de Rives bags were inserted, the cervix dilated and then, after catheterization and chloroform anesthesia, the forceps were applied.

The head was engaged, but high in the pelvic canal. The in-

strumentation lasted an hour, no help being afforded by uterine contractions. The forceps were not taken off during this time, but frequently unlocked to relieve pressure.

When the head was brought down on the perineum the fetal heart was so weak that it became evident that either the fetus or the perineum must be sacrificed and the latter was chosen.

The perineum was torn to the second degree. The cord was once about the body and the child was deeply asphyxiated. The child was, however, resuscitated by slapping and applying cold water to the nape of the neck and blowing upon the neck and face. At last it cried and later became ruddy, a perfectly good color. It is seen that the common method of resuscitation by artificial respiration by doubling up the child was avoided, as this procedure can but increase the pressure within the cerebral vessels, which have already been submitted to such an extraordinary strain.

The placenta was expelled by the Crédé method after forty minutes with membranes intact and no postpartum hemorrhage ensued. The perineum was repaired, but all the stitches came away, no effort at healing being evident for nearly two weeks.

Here certainly were two potent factors in the induction of birth hemorrhage, prolonged labor and instrumentation.

The child showed superficial excoriations over the left ear and neck and right eye. There were extensive hematomata of the parietal regions of both sides.

The position had been R. O. A. The delivery had been accomplished at 3:30 P.M. That evening the child showed a peripheral facial palsy of the left side, being unable to close the left eye and displaying a marked flattening of the left side of the face and drew the mouth well to the right on crying.

The next morning the eye could be closed and the facial palsy was less marked, but the left arm was found very rigid and the hand dorsally flexed at the wrist, the hand approximating the position of *main en griffe*. The left leg was also rigid and the foot sharply flexed dorsally. The knee-jerk on that side was greatly exaggerated. That evening the condition of the left side was the same and convulsive movements, especially of the upper part of the body, were manifest, with marked shrugging movements.

The color was still good at this time, thirty hours after birth, and the pulse still strong, an important point to note. The child cried a great deal that day.

By the next morning a decided change had taken place. The child looked badly and was cyanotic. It was decided to have recourse to an operation, and Dr. Alfred S. Taylor was called in, whose report on the procedure and discussion from the surgeon's standpoint follows.

In this case the operation consisted in turning down an osteoplastic flap, which comprised most of the parietal bone of the right side, with the attached base just above the ear. A slightly smaller dural flap was turned down, exposing the surface of the brain covered by blood clot. This clot covered not only the whole area exposed, but also extended beyond the cut edge of the dura on all sides. It was comparatively thin, superiorly and posteriorly (about $^1/_{16}$ inch), but became thicker toward the lower anterior angle of the wound at the beginning of the fissure of Sylvius. At this point active bleeding was present, but whether it was a part of the original process or the result of operative interference could not be definitely determined.

All of the clot that could be reached was gently removed, the brain surface was irrigated with warm saline solution, and the fresh hemorrhage was controlled.

The dura was closed by a continuous catgut suture; the osteoplastic flap was laid back in place, and the periosteum was sutured with chromic catgut; the scalp wound was closed by a continuous suture of silk. Sterile dressings were applied and the patient returned to bed.

In spite of the removal of the clot there was no improvement in the baby's condition, and about a half hour later death occurred.

Postmortem examination of the brain revealed that in addition to the clot removed at the time of operation, an extension of it passed along the inner end of the fissure of Sylvius to the base of the brain, backward beneath the right temporo-sphenoidal and occipital lobes, well forward beneath and between the frontal lobes, over into the beginning of the left fissure of Sylvius, backward beneath the left temporo-sphenoidal lobe, and finally down into the posterior fossa of the skull, where the anterior surfaces of the pons, the medulla, the cerebellum and the spaces between them were covered with thick clot.

The last-described portion of clot undoubtedly caused death by pressing upon the vital centers.

The whole convex surface of the brain was markedly congested, more especially upon the right hemisphere.

While it cannot be determined just when the clot made its appearance in the posterior fossa, nor just when it formed, the continuity of this clot with that in the middle fossa, and the fact that the signs of pressure upon the vital centers in the medulla appeared only during the very last few hours of life, justify the belief that it was crowded down from the middle fossa as a result of the increasing pressure there present.

If this assumption be granted, this case very strongly emphasizes the fact that, if operation is to succeed in these cases, it must be undertaken at the earliest moment the diagnosis can be made.

DISCUSSION.

DR. TAYLOR.—The specimen (exhibited) shows very clearly the beginning of a hemorrhage around the Silvian fissure; its extension up on the cerebral surface is not so evident. The extension of the clot, however, through the fissure toward the base of the brain, under the frontal lobes, and backward under the temporosphenoidal and occipital lobes, and the marked extension downward behind the sella turcica, is very definite and the clot is absolutely continuous with no break, so that we felt justified in assuming that the hemorrhage occurred at the Silvian fissure. The focal symptoms in relation to the arm appeared first. The relief of pressure on the cerebrum prevented further convulsive movements. The symptoms, with relation to pressure upon the bulb, were uninfluenced; they continued with increasing severity until the child ceased to breathe.

In doing an operation for the relief of fresh hemorrhage it is important that the operation be as brief as possible and that the interference should cause the minimum amount of traumatism to the brain tissue. The traumatism which has already occurred causes the brain to be very easily injured, and in cutting through the bone one cannot use rough measures without producing further traumatism to the cerebral structures. One cannot use the scalpel for fear of cutting through the dura. The saw cannot be used for the same reason. Strong blunt scissors are recommended by Cushing. (Dr. Taylor exhibited instruments devised by him for this operation, with which injury to the brain substance he thought was less likely to occur.)

The cases naturally which are best for operating are those where the clot is confined to the upper surface of the brain, because there you get a good deal of exposure and can remove the clot. These hemorrhages are practically always on the lateral or midportion, under the parietal bone, and cause pressure on the

motor area, so that motor irritation is an indication for operation. Where the clot is on the base of the brain the symptoms are those of pressure on the vital centers. In these the prospects are not so good. One cannot handle the soft, delicate brain with sufficient ease to make sure of getting out all the clot without doing injury to the cranial nerves. There is also the serious question of whether such a child, if it were to live, would be a useful member of the community, because there might still be enough clot to cause interference with function.

Operative procedure, to be successful, should be undertaken at the earliest moment, as at that time there is the least amount of injury from the hemorrhage and the child has the best resistance.

DR. LA FÉTRA.—I desire to emphasize the fact that when the cord is around the neck one has to be all the more on the watch for symptoms of cerebral hemorrhage occurring a shorter or longer time after birth. I had occasion last year to look up a number of records of cerebral hemorrhage, and in a large number of the cases the cord had been around the neck.

DR. KOPLIK.—It must be very difficult with some of these children to decide whether operation should be done or not for two reasons. We all know that a great many of these children grow up in a deficient state mentally and physically, but we also know that a great many of these infants do not. I have in mind one child in whom there were all the symptoms of hemorrhage, and still the child is today a most beautiful girl with no bad symptoms whatever. So it is a matter for grave consideration whether to undertake the operation or not.

DR. MORSE.—It has been my misfortune to see a number of these cases during the past winter, and I have been struck with the irregularity of the symptoms and with the late development of the symptoms in some of them. I had previously supposed that they came on within the first twelve hours, but have recently seen cases in which they did not appear until the third or fourth day.

It must also be remembered that other conditions may give rise to symptoms very similar to those of cerebral hemorrhage. Even the motor symptoms may be due to conditions entirely outside the head, gastrointestinal, for instance.

Personally, I feel very cautious about making the diagnosis and recommending an operation. It is a very serious thing to recommend an operation as grave as this when a dose of castor-oil would cure the trouble.

The symptom which I have found most constant is bulging of the fontanel. If the fontanel bulges there is almost certainly a hemorrhage. Another symptom which I have seen in nearly all of my cases has been bleeding from the nose or back of the

throat, the blood presumably coming through the cribriform plate. Another means of diagnosis is, of course, lumbar puncture. A bloody fluid points strongly to cerebral hemorrhage, but is not conclusive evidence, as the blood may come from a wounded vein. Too much reliance cannot be placed on it therefore.

Even when you have made up your mind that there is a hemorrhage it is difficult to decide on which side of the brain the hemorrhage has occurred and as to whether it is cortical or at the base. Dr. F. T. Murphy has called attention to the presence of a bluish color showing through the fontanel after the deflection of the scalp as being of some importance in localizing the clot. Another difficulty is in determining whether, after the clot has been found and removed, all the hemorrhage has been found or whether there has been another somewhere else.

I have been much impressed with the difficulties and dangers of the operation, and the more I see of these cases the more surprised I am at Dr. Cushing's results in his first series.

DR. HOLT.—When one sees these children when they are twelve or fourteen months old, or later, with spastic paraplegia and mental defects, one feels justified in attempting almost anything, and yet I feel very much with Dr. Morse that the difficulties of diagnosis and operation are great. In the first place, nearly all these hemorrhages are diffuse and one cannot be sure he is getting all the clot out.

I think, also, that bulging of the fontanel is the best single symptom in diagnosis. A slow pulse is also significant. No importance, however, should be attached to obtaining bloody fluid by lumbar puncture. We get that every now and then in puncture in all sorts of conditions. To be certain of our diagnosis we should have localized symptoms, hemiplegia or convulsions of one side.

I have seen several cases that had very high temperature. One child we watched very closely, and for four or five days its life was despaired of; temperature, 105° F. for two or three days. I have known other cases with fairly marked symptoms to come to operation and nothing found. One such case I saw recently which Dr. Cushing operated on. The symptoms were of a general character, recurring convulsions and rigidity. I think that we are really very much in the dark at the present time, and, considering the danger of operation and the difficulties of diagnosis, I am not very hopeful as to the benefits to be derived, though I should feel like having the operation done in every case in which there were focal symptoms.

DR. MILLER reported a case seen by him the past summer. Six days after birth the child was seized with general convulsions and rigidity, which lasted five days. During the remainder of its life it would have retraction of the head, amounting, at times, to opisthotonos whenever it was irritated or excited. Then

it contracted whooping-cough, and died at eight months. Dr. Spiller saw the case a couple of times, and was of the opinion that there had been, possibly, a hemorrhage of the brain. The child came to autopsy, and the brain was macroscopically normal, but the microscope showed diffuse meningitis. The child presented all the symptoms of a diffuse hemorrhage, although none was present.

DR. FREEMAN.—In connection with the remarks concerning the symptom of bulging of the fontanel, I would speak of a case I saw a short time age, where the child had such bulging, with focal symptoms, and operation was advised but refused. I saw the child afterward and it had improved very much, the symptoms having practically disappeared.

DR. CHURCHILL.—When we consider what happens to most of these children who live I feel, with Dr. Holt, that one is justified in advising almost anything, and, personally, if I had a child in this condition, I should insist upon having the operation done. As I remember Dr. Cushing's paper, the 2 cases surviving were recognized and operated upon early, while the 2 that died were not operated upon until after a considerable time. I happen to have seen 4 of these cases during this last winter, all in consultation; 3 of them were moribund; the fourth we might have operated on if the parents had been willing, but they were not; and even the attending physician was very lukewarm about it. There should be a campaign of education carried on concerning this subject, pointing out that physicians should be very careful in looking for cerebral symptoms in cases of difficult labor, and that at the first sign of such symptoms a surgeon should be called in for an opinion.

DR. HAMILL.—I am quite in sympathy with what Dr. Holt has said as to the difficulty of diagnosing these cases. I have seen quite a number and have followed many of them to the autopsy table. Some cases, which have closely simulated this condition, have shown no evidence of hemorrhage at the autopsy. In cases with definite localizing symptoms I have had no difficulty in making a correct diagnosis; it has been in the cases where these did not exist that errors have occurred. In the cases reported there were localizing symptoms; and, as I understood the paper, it was in cases of this sort only that Drs. Meara and Taylor considered the operation justifiable.

DR. HOWLAND.—Even when there is a mistake in diagnosis in these cases it does not mean that the operation is necessarily fatal. In one of Dr. Cushing's cases he went in on one side and found nothing; went through the other parietal bone and found nothing; went through the occipital bone and found nothing; and yet the child's condition was not prejudicially affected even by such an extensive operation.

DR. KERLEY.—I hope some day that we will have better means of diagnosis in these cases. I had a case recently where three days after birth the child had convulsions and complete hemiplegia. The child was seen by different men, each of whom made a fatal prognosis, but the child recovered perfectly. In this case all the symptoms spoken of were present.

DR. TAYLOR (closing remarks).—I would not like the members to think that we supposed every case should be operated upon. We want to make it clear that it is in the cases of focal lesion that the opportunity for recovery is best. In a subject like this, where surgical intervention is a new thing, it is necessary that the subject should be brought up frequently, just as it was in the case of the surgery of the appendix. Some men thought every child should lose its appendix lest appendicitis should develop later; other men thought they should not be operated on at all. Now we are all pretty well agreed as to the indications for that operation, and so it will be with this condition after a while. In some, operation is indicated from the very start, because it offers the only hope. There are others which are debatable, and it is in these that a discussion of the differences of opinion will put us upon the right track. As to the danger of the operation itself, it is apt to be magnified. The impression is abroad that the young child doesn't stand operation well; that is incorrect.

DR. MEARA (closing).—We think we have sufficient anatomic evidence to show that the delay permitted further leakage and pressure on the vital centers.

CONGENITAL CYST OF THE LIVER, WITH REPORT OF A CASE.

BY HENRY L. K. SHAW, M.D.,
AND
ARTHUR W. ELTING, M.D.,
Albany, N. Y.

An exhaustive review of non-parasitic cysts of the liver was made by Moschowitz in 1906, and he was only able to find 85 cases in the literature. Since that date we have discovered 3 more reported cases, making a total of 88. Two of these occurred in the fetus, 7 in the newborn, 4 in the first year, 1 eleven years of age, and the remainder in adult life. By far the largest number of cases were in persons over sixty years of age, and Claude, a French author, goes so far as to state that cysts of the liver are found exclusively in persons of advanced age. Multiple cysts were found in all the cases occurring in infants, and in every case there were malformations or abnormalities in other organs—most commonly cystic degeneration of the kidneys.

The following case of a single non-parasitic cyst of the liver in an infant, with operation and histologic examination, is of such unusual occurrence as to merit its presentation before this Society.

Priscilla M. B. was referred to Dr. Shaw by Dr. Woodruff, of Pittsfield, to whom we are indebted for the very complete history. She was born July 20, 1907. The labor was normal and uneventful and the birth weight was 9½ pounds. The mother was thirty-eight years old, had mitral regurgitation and was not very strong. The father was forty-five years old and had chronic nephritis. There was no history of alcoholism or syphilis. For nine months she was fed exclusively on the breast, and at three months (?) she weighed 19 pounds. The child was perfectly healthy and well until July, 1908, when she had a severe attack of ileocolitis. At this time, when examining the abdomen, Dr. Woodruff first discovered a small but distinct mass at the right costal border of the ribs, which he considered an enlargement of the right lobe of the liver, due to the intestinal infection. The child recovered after a few weeks and no more attention was paid to the enlarged liver. The color improved and there was a marked gain in weight after this illness. She contracted measles

at the end of August, when thirteen months old, and made an uneventful recovery. A few weeks later the prominence of the right side of the abdomen became more pronounced and it continued to increase in size. There had never been any jaundice, pain, or any subjective symptoms referable to the liver. The appetite was excellent and the child was notably bright, intelligent and playful.

The child was brought to Albany for the first time on December 14, 1908, when she was seventeen months old. She is a well developed child, weighing 31 pounds. She has had four meals a day of a plain, simple diet, with plenty of milk. The appetite has been good and the bowels regular. The mother stated that she first noticed the "lump" last August, when it seemed about the size of an orange. She is certain that it became much smaller, but has grown rapidly larger for the past four weeks.

The color is good and there are no evidences of rickets. The circumference of the head is 18¼ inches, of the chest 20½ inches, of the abdomen just above the navel 22 inches. The examination of the lungs and heart is negative, and the lymph nodes are not enlarged. There is no jaundice of the skin or conjunctiva. The abdomen is greatly distended, but the superficial abdominal veins are not enlarged and the umbilicus is depressed. The upper border of liver dullness is at the upper border of the fifth rib, and below it extends to the right iliac fossa and to the median line. The edge is sharp and the surface smooth and free from nodules. Percussion and deep palpation are not painful and the baby evidenced no discomfort during the examination. The left side of the abdomen is tympanitic and there are no evidences of fluid or masses. The extremities show nothing abnormal.

No positive diagnosis was made. The whole appearance of the baby, the physical examination and the history, seemed to exclude sarcoma or any malignant growth of the liver or kidney. There was no edema or ascites and the spleen was not enlarged, so the condition could not have been due to an amyloid liver. There were no evidences of syphilis. The persistent absence of jaundice would exclude fatty liver or hypertrophic cirrhosis. That the possibility of cyst of the liver was not considered is perhaps not to be wondered at, as an eminent surgeon has stated that cysts of the liver belong to the unexpected in surgery. An exploratory incision was deemed both advisable and justifiable, and the child was referred to Dr. Elting.

SURGICAL REPORT BY DR. ELTING.

On admission to the hospital the patient presented a marked enlargement of the entire abdomen, which was somewhat asymmetrical, the right hypochondrium being decidedly more prominent. On palpation a very distinct tumor mass could be made out, which occupied the entire upper right quadrant of the abdomen and extended downward to the level of the umbilicus. The tumor was rather regular in outline, somewhat globular in shape, and appeared to be directly connected with the liver and the consistence was rather firm. The dullness of the liver was absolutely continuous with that over the tumor. There seemed to be a slight movement of the tumor with respiration. There was no tenderness on pressure. The tumor did not appear to extend to the left farther than the median abdominal line. Because of the child's excellent general condition, an exploratory laparotomy was deemed advisable. This was done under ether. Through a linear incision extending from the costal margin to the level of the umbilicus, about 3 cm. to the right of the median line, the abdomen was opened. The liver was found to be somewhat enlarged. The lower part of the right lobe seemed to be occupied by a globular tumor which protruded downward to about the level of the umbilicus. On palpation there seemed to be definite fluctuation in the tumor and the surface of the tumor at the lower portion was covered by a zone of liver tissue, approximately ½ cm. in thickness. The tumor was punctured and about 900 c.cm. of a clear yellow fluid withdrawn. The opening into the cyst cavity was enlarged and a rubber tube and vioform protective drain introduced into the cavity. The wound was closed with interrupted silk-worm gut. The patient stood the operation, which lasted about eighteen minutes, extremely well and left the table in good shape. There was practically no loss of blood and no evident shock. She continued in good condition for approximately two hours, when she suddenly died. As revealed by the postmortem there was no hemorrhage and no other definitely assignable cause for the death other than shock.

A careful examination of the fluid was made. It was found to contain a moderate amount of albumin and microscopically it showed only detritus; no cells or formed elements of any variety were found.

PARTIAL AUTOPSY DESCRIBED BY DR. WOLBACH, OF THE BENDER HYGIENIC LABORATORY.

Liver.—Weight, 625 grams. There is a large, thick walled cyst situated approximately in the median portion, but apparently taking origin in the central portion of the right lobe and extending about 3 cm. into the substance of the left. The superior and anterior portions of the cyst wall are thin and fibrous in appearance. It varies in thickness from .3 to .6 cm. At the anterior border the cyst wall is covered with a layer of liver tissue .2 to .5 cm. in thickness. Inferiorly the cyst wall consists of fibrous tissue with no suggestion of overlying liver tissue except at the

I. ANTERIOR VIEW, SHOWING CYST WALLS HELD APART BY GLASS RODS.

Spigelian lobe. The latter is thin and concaved to fit the spherical curvature of the cyst wall. All external surfaces of the cyst are smooth and covered with peritoneum.

The gall-bladder lies in a deep fissure to the right of the cyst and contains a pale yellow, slightly viscid, transparent bile. The diameter of the cyst, which is nearly spherical, is 10 cm. The greater part of the inside is lined with a smooth glistening covering. In a few places there are remains of old hemorrhages beneath the surface. A large portion of the cyst wall contains recent hemorrhage upon the surface and beneath the surface of the lining. The liver tissue is pale reddish-grey in color. Consistency is slightly increased. Cut surfaces have a rather refractive appearance. Lobular outlines are prominent, owing to

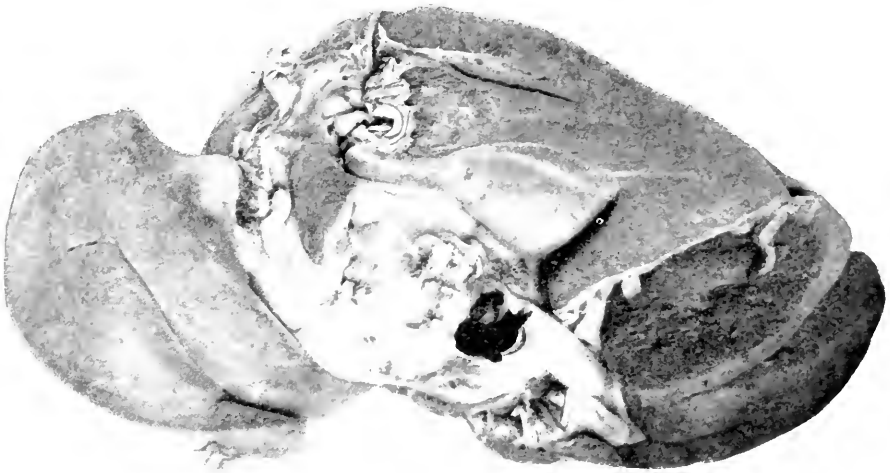
injection in central veins. Peritoneal surfaces of both lobes are slightly injected.

Kidneys.—Apparently normal.

Spleen.—Apparently normal.

MICROSCOPIC EXAMINATION.—*Liver.*—There is a considerable increase of connective tissue throughout the liver in the portal spaces. The arrangement of lobules is normal and the liver cells are normal. The connective tissue of the portal spaces is dense, and contains tortuous bile ducts and occasional groups of atrophied liver cells.

The cyst wall (six places examined) consists of dense fibrous tissue without a lining layer of cells. The inner surface in many



II. POSTERIOR VIEW.

places is necrotic and contains fibrin between the cells and bundles of collagenous fibrils. In other places there are hemorrhages into the cyst wall and on the inner surface. In a few places there is granulation tissue growing into the foci of hemorrhage. Externally the cyst wall is continuous with bands of connective tissue extending between the lobules into the liver substance. These bands of connective tissue are concentrically arranged to the cyst wall and contain atrophied islands of liver cells and tortuous bile ducts.

Spleen.—Negative, except for marked injection and slight hyperplasia of germinal centers in the secondary follicles.

Kidney.—Negative.

Epicrisis.—In the absence of a lining membrane or islands

of lining cells to the cyst wall it is impossible to advance hypotheses for its origin. The irregular thickness of the wall is due to organization of small hemorrhages. The connective tissue increase in the adjacent liver substance is probably reactionary to pressure.

Numerous theories have been advanced to explain the formation of liver cysts. Ribbert calls them enterocysts and finds the cause in an embryonic malformation of the mesoblast. The liver is developed in the anterior mesentery and its mesoblastic elements have a common origin with the diaphragm. Its ducts and glandular elements are derived from a sprout from the duodenum. The liver, therefore, is an outgrowth and appendage of the alimentary tube. The early embryonal buddings from the primitive digestive tract with the involved infolds from the primary embryonal plates are not yet distinctly understood. Zahn collected 14 cases in adults with single cysts of the liver in which the cysts were lined with ciliated columnar epithelium. These cysts, without exception, were situated on the anterior surface of the liver in the neighborhood of the falciform ligament. He points out the fact that ciliated columnar epithelium is normally never found in the liver.

Still believes the cyst formation is a congenital malformation and not a disease or result of inflammation. They cannot be due to dilated bile ducts or to some obstruction, as jaundice is never present. He says: "The liver condition is not a progressive one, and the subjects of this malformation can live to old age with no symptoms referable to the liver, unless the distention produces mechanical effects, which very rarely happens."

Moscheowitz reached the conclusion that these cysts are always associated with congenital anomalies of the liver, consisting in aberrant bile ducts, which may be extrahepatic or intrahepatic. The cysts have their origin in these aberrant ducts and may assume two forms: One arising from inflammatory hyperplasia of these ducts, the other by retention of fluid in these ducts, the result of congenital obstruction.

Our case, unfortunately, does not offer any solution as to the etiology of cysts of the liver. From a diagnostic standpoint, it presents several interesting features, and the possibility of this condition in infants must be kept in mind when obscure abdominal conditions present themselves.

THE SO-CALLED THYMUS DEATH, WITH AN ACCOUNT OF SEVEN CASES OF SUDDEN DEATH IN ONE FAMILY.

BY J. P. CROZER GRIFFITH, M.D.,

Clinical Professor of Diseases of Children in the University of Pennsylvania, Philadelphia, Pa.

The following history is one of the most interesting and unusual instances of the so-called thymus death of which I have read or heard; certainly the strangest which has been in any way under my observation. One other somewhat similar account in medical literature has come to my notice—that given by Hedinger,[22] in which 5 children of one family had died suddenly with symptoms alike in all, one autopsy showing an enlarged thymus gland.

I was called early in April, 1907, in consultation to see a child of three months, born of Italian parents. Nothing ailed the infant at the time except a slight bronchitis, and Dr. Theodore Sprissler, the attending physician, had asked for the consultation in order to discuss means for the prevention, if possible, of the fate which had occurred to the majority of the preceding children. The notes kindly furnished me by Dr. Sprissler, combined with my own questions and observations, and the autopsy-report by Dr. Howard Childs Carpenter, constitute the basis for the following history:—

The antecedents on the paternal side, as far back as the grandfather, exhibited nothing which could explain the later events in any way, and the same was true of the maternal antecedents. The father and mother of the patient were apparently perfectly healthy individuals. The first child was born prematurely at seven months, and died after twenty-four hours. The second child was stillborn. The third child died at three months with symptoms said to resemble entirely those seen in the later-coming children. The diagnosis of "pneumonia and heart failure" was given by the attending physician, but the cause of death was evidently very obscure. The fourth child died at six months with the same symptoms as those seen in my patient. Here, too, the exact cause of death was certainly unknown, although the diagnosis of "pneu-

monia and weak heart" was made. In neither case was there an autopsy. The fifth child was living and well at the age of eight years. The sixth child died at five and a half months. It had had a slight bronchitis, from which it had recovered. A tonic was then prescribed by the physician, an able man well known in Philadelphia, but the child died suddenly while the first dose of medicine was being given to it. The seventh child died at eight months from "pneumonia and bronchitis." The symptoms were the same as those to be described in the case of the ninth child. The eighth child died at one month; and here, too, the symptoms were identical with those seen in the other children. The cause of death was, as in the others, evidently obscure. The consultant who was called in thought, it is stated, that the bronchial tubes were too small to allow the air to enter the lungs. Some degree of bronchitis preceded death. The ninth child is the case now to be reported. Its symptoms are said to have been exactly like those of the others. A tenth child has since been born, but died at the age of ten weeks with the same symptoms as seen in the others.

The ninth child, Albert S., was born December 31, 1906, in normal labor, without unusual symptoms, except for a slight degree of asphyxia, traces of which lasted a few hours. He continued in good health after this period and was hearty and well nourished, but on account of the dread of the fate which had befallen the others, the parents requested Dr. Sprissler to make weekly examinations. This he did, finding nothing wrong at any time except a few râles in the chest. The child was breast-fed for the first three weeks and was then weaned gradually. About the end of March, being then three months old, he experienced a slight increase in the bronchitis, and I was asked to see the case in consultation, as I have already stated, not because he was really ill, but to aid in preventing, if possible, a repetition of past experiences.

I found the infant well nourished, with no evidence of illness except for a few scattered, coarse râles in the chest and a slight cough. The diagnosis of bronchitis of a mild degree was made, with a secondary diagnosis of a possible status lymphaticus, based entirely on the family history, and not upon the appearance of the patient. The use of atropin was advised, together with counter-irritation of the chest with mustard plasters.

I never saw the baby again. He continued in good condition until April 13th, on which date the cough increased. On the

morning of the 14th his lips and extremities were livid and auscultation showed harsh respiration on both sides of the chest, with coarse râles. Percussion showed nothing abnormal. By evening the cyanosis had become widespread and intense and there was dyspnea. These symptoms increased until the child died, at 9 A.M. on April 15th, the severe symptoms having lasted twenty-four hours. There were at no time any manifestations which suggested tracheal stenosis.

I condense from the notes of the autopsy:—

The thymus gland was large and thick, extending downward as far as the auriculo-ventricular septum. It appeared to press upon the great vessels. Its dimensions were 7 cm. in length, 4 cm. in width, and $2^1/_2$ cm. in thickness, and its weight 27.5 grams. On section it showed a simple hyperplasia, with no evidence of excessive secretion, edema or unusual congestion. There was slight beading of the costochondral articulations. Both lungs showed hypostatic congestion of the lower lobes, but were otherwise normal and were crepitant throughout. The heart was large, dilated, hypertrophied, without evidence of valvular lesions; the aorta normal; the bronchial and mesenteric lymph glands enlarged; the spleen moderately enlarged and congested on section.

We have here, then, a strange family history—eight children, omitting the stillbirth and the premature birth, all but the ninth breast-fed, and only one of them alive at the time of writing. The other seven died between the ages of one month and eight months, all well nourished, with uncertain diagnosis, and all with clinical histories nearly identical, namely, slight bronchitis followed in one instance by extremely sudden death, in the remainder by the rapid development of cyanosis and dyspnea, lasting some hours or a day, but without any symptoms suggesting stenosis, the condition in the ninth child being proven by autopsy not to depend upon pulmonary involvement. In the last case, too, a very large thymus gland was found. It is a supposition only, but a warrantable one, that with the clinical symptoms the same, and in view of the family relationship, the lesions also in the other children were the same as in the ninth one.

As to the cause of death in these cases there is, I think, no evidence that tracheal compression existed in any of them. Not only was there no appearance of this at the autopsy upon the ninth child, but the symptoms in no instance were those of stenosis of the trachea. The autopsy excluded, too, the influence of bronchitis

and pneumonia in the ninth infant, and presumably in the others, as their symptoms were the same, and certainly not those usually seen in these diseases. The sixth child suffered an absolutely sudden death suggestive of sudden cardiac inhibition. In the others the failing circulation, which appeared to have been the cause, came on more gradually. The thymus gland in the ninth case seemed at autopsy, as stated, to compress the great vessels, but no positive proof of such compression was present. That there was actually any such compression exercised I doubt, for reasons which will be spoken of later.

The subject of the condition to which the title "thymus death" has often been applied has been so much discussed in the last few years that I would hesitate to approach it again were it not that some recent writers, notably Warthin[45] and Rehn,[39] take the ground that it is now definitely proven that the majority, if not, indeed, all, such cases are undoubtedly due to pressure by the enlarged thymus gland and that the older theories of status lymphaticus, or other conditions, as a cause of death, are practically exploded. This is so far from my own views and those of many others who have studied the subject, that a brief discussion of it may perhaps be of value.

As far back as 1614 Plater[36] noted the frequent presence of enlargement of the thymus gland in cases of sudden death, and the association was frequently observed after this date. Finally, in 1830, Kopp[30] exploited the theory of "thymic asthma" as a result of thymic enlargement. The belief in this association became widespread, although still much disputed, until, in 1858, Friedleben,[17] as a result of extensive studies, discredited Kopp's theory and denied the existence of any relationship whatever between enlargement of the thymus and asthma or sudden death. This view found general acceptance for thirty years, until, in 1888, Grawitz[20] advanced the belief in death from compression of the trachea by a large thymus gland, based upon autopsies in a couple of cases, although in neither of them was compression of the trachea found postmortem. The weight of his authority was great and convinced many. A year later, however, in 1889, Paltauf,[34] as a result of many autopsies, proposed the theory of the "status lymphaticus," which found a ready acceptance. In 1896 Escherich[10] adopted this theory in a modified form, believing that with the gross anatomic lesions of this condition there was developed a "neurosis," the result of an autointoxication, upon

which death depended, the fatal ending being caused by inhibition of the heart's action. Since this time the subject has been one of enormous dispute, chiefly because it is one in which it seems impossible positively to prove anything in many instances. The several theories advanced may be classified as follows:

A. *Nonmechanical, or Convulsive.*

(1) A hyperthymization of the blood, the result of overproduction of the thymic secretion, a toxic condition resulting.

(2) Defective secretion of the thymus with consequent constitutional disturbances.

(3) A toxic action of some other nature not definitely known.

(4) A neurosis of some sort through which the nervous system is rendered extremely sensitive, and cardiac inhibition follows from slight and often undiscoverable causes, and which has thymic enlargement and other lesions of the status lymphaticus as its anatomic manifestations.

(5) A neurosis of this nature, *independent* of any so-called status lymphaticus.

B. *Mechanical.*

(6) Pressure upon the trachea, producing reflex inhibition of the heart's action through irritation of tracheal nerve filaments.

(7) Pressure upon cardiac nerve trunks or the great vessels.

(8) Direct compression of the trachea with consequent strangulation.

As to the method by which the thymus exerts pressure, under the heading "mechanical" there are various opinions, viz., the pressure may be produced (1) by swelling due to retention of its own secretion; (2) by edema; (3) by congestion; (4) by bending back of the head and consequent sudden narrowing of the space occupied by the trachea and the enlarged thymus; (5) by sudden shifting of the position of an already enlarged thymus into the "critical space" of Grawitz, and consequent pressure on the trachea.

Only two views have received acceptance at all widespread. First, that there is a neurosis of some sort, toxic or otherwise, and that death is a cardiac death. Second, that there is compression of the trachea, and that death is due to strangulation.

Death from pressure on the large vessels is not believed in by many observers and has much against it, chiefly that the vessels, especially of the venous trunks, are situated rather low to receive injurious pressure, and that it is more likely that the soft thymus

would be indented by the pressure of the aorta than that the gland itself would exert compression. So, too, there are not many who accept the theory that death is due to pinching of the nerve trunks leading to the heart.

The subject has been one of much interest to me, and I have kept a fairly careful watch on its literature, and have myself discussed it in an earlier publication. There is no question that death by pressure of the enlarged thymus gland upon the trachea can, and does, occur. Autopsies showing this compression and indicating the possibility of death being produced by it have been reported by Beneke,[4] Marfan,[32] Barack,[3] Clessin,[7] Landge,[21] Jessen,[27] Farret,[11] Hedinger,[23] Flügge,[14] Tailleus,[45] Feer,[12] Penkert,[35] Baginsky,[2] Gluck,[19] Birch-Hirschfield,[6] Somma,[44] Paltauf,[34] Biedert,[5] Abelin[1] and Wiegert.[49] There are probably others which I have overlooked. Abstracts of the pathologic findings of most of these cases are given by Hotz. As to the clinical conditions present, it is to be noted that, whereas in some of the cases, as far as I have been able to consult the original publications, there was during life decided evidence of tracheal stenosis in a large number, there was not satisfactory evidence that compression had been the actual cause of death.

More important is the fact that operation has been performed in a number of cases with more or less complete excision of the thymus, sometimes with anchoring the remainder, and prompt relief has followed. Such cases have been published by Siegel,[42] König[29] (2 cases), Purrocker,[37] Ehrhardt,[8] Hinrichs,[24] Rehn[39] and Schwinn;[41] and 1 case, that of Friedlander,[16] was relieved by treatment with the X-ray. Perhaps here should be placed one of Abelin's cases in which tracheotomy afforded prompt relief. A sudden subsequent attack was fatal and a very large thymus was found at autopsy. Nothing, however, is said of the discovery of compression of the trachea by it having been observed.

This makes 20 reports showing evidence of pressure postmortem, and 10 other cases relieved by treatment. There we stop, leaving the large number of other published cases not satisfactorily accounted for on this theory, as, indeed, is true of many of those in the list of cases with compression found postmortem.

As to the majority of the numerous reports of "thymus death," or with other headings indicating death from pressure, I am forced to the conclusion that many of them are merely instances of imperfect logic. A child dies suddenly; autopsy shows an en-

larged thymus gland; conclusion, death is due to enlargement of the thymus. We observe, however, that no satisfactory postmortem evidence is offered that the thymus had anything to do with the death, and the numerous cases in which sudden death has occurred and no thymic enlargement been found are ignored, as are the equally numerous ones in which a much enlarged thymus has been discovered in cases dead from known causes of some other nature. From the major premise that "some enlarged thymus glands cause death," the reporter concludes that *all* deaths where enlargement of the thymus is present are due to this cause. He does not offer proof of this.

In all the 10 cases relieved by operation, or in other ways, it is noteworthy that symptoms of tracheal stenosis, gradually increasing, had lasted for weeks or months. The matter is very different in the majority of cases of "thymus death" reported. Here the child dies suddenly, or after a few hours only, with evidences of failing circulation. The death in some instances appears to be from spasm of the larynx, but is, in reality, as generally admitted, cardiac, the heart's action ceasing first. Moreover, autopsies show, as Targhetta[46] has pointed out, a striking absence in many of these cases of any evidence of asphyxia. Indeed, no satisfactory cause of death is discovered either in the symptoms or in the postmortem lesions.

In fact, to anyone reading the literature of the subject carefully, it seems to me it must be evident that there are at least two classes of cases—those dying from mechanical obstruction to respiration and those dying from nervous or other influence with entirely different symptoms indicating cardiac failure, and with no evidence of tracheal stenosis.

Those who oppose this view are forced to advance the theory that the thymus is capable of undergoing extremely sudden alteration in size. As to the possibility of such sudden enlargement, it is chiefly theory, with little anatomic or physiologic support, and much which is directly opposed to it. Anatomists are agreed that the arterial and venous supply of the thymus is comparatively small, and this makes it difficult to admit the possibility of a congestion more sudden than in any other organ. Although congestion of the thymus gland has repeatedly been found at autopsies, it has never been proven to have arisen suddenly or to have been excessive, and it has also repeatedly occurred that no such hyperemia has been present in cases of sudden death associated

with thymic enlargement. Moreover, Clessin[7] attempted to produce such congestion experimentally in dogs without results. The possibility of death being due to the bending of the head backward, and the consequent compression of the trachea by an enlarged thymus gland, as urged by Beneke,[4] is negatived by the fact that bending the head forward again does not relieve the trouble; as also by the fact, pointed out by Tailleus,[15] that cases sometimes die suddenly without any changing of the position of the head. Then, too, Simon and d'Œlsnitz[13] injected melted paraffin into the trachea of dead children, placed an enlarged thymus gland in position and then attempted to produce indentation by bending the head backward, but without result sufficient to warrant the belief that interference with respiration could be produced in this manner. There has, as far as I know, been no proof advanced of the existence of sudden decided enlargement by retained secretion or from edema, with consequent tracheal compression.

Even if we were to admit the possibility of more or less sudden swelling from some cause we should still fail of explanation for death with symptoms of sudden heart failure, which is so characteristic of many of these cases, or for the absence of progressively increasing dyspnea as the gland grew larger. With these facts in view the assumption that the thymus suddenly swells to an extent capable of causing death, seems to me lacking of satisfactory proof.

Certainly as far as my own experience and reading of literature goes, I feel convinced that whereas there are cases in which death results from pressure of the thymus gland upon the trachea, these are exceptional, and warning in such cases is given by symptoms of strangulation gradually increasing in severity. The cases of actually sudden, or at least rapid, death, I think depend upon a neurosis which may be associated with lymphatic and thymic enlargement as its anatomic manifestations, but is not necessarily so. It is only on the supposition of some such neurosis as a result of which cardiac inhibition takes place from insignificant causes, that a reasonable explanation is to be found for the sudden death sometimes occurring in eczema, as especially studied by Feer,[12] following plunging into cold water, as reported by Nordmann[23] and others, the giving of hypodermic injections, the puncture test of the pleural cavity, and the like.

As an indication that the opinion expressed is one shared by

many who have studied the subject, let me quote further, briefly, from some well-known, comparatively recent writers.

Friedjung,[15] in 1900, made a most extensive and much quoted critical study of the subject with 116 references to its literature. As a result he concluded that undoubted cases of pressure of the thymus upon the trachea do exist, although whether upon other organs cannot yet be determined; and he believed, further, that the bending back of the head was without influence. He admitted the existence of the status lymphaticus and the tendency of sudden death depending upon this. In his recent article in Pfaundler and Schlossmann's handbook he reiterates this opinion, which the publications of others since his earlier contribution have not changed; emphasizes the necessity of the symptoms coming on gradually in cases of compression, and claims that there is no proof of the occurrence of sudden swelling of the thymus gland.

Thiemich,[47] in 1901, carefully analyzed the various causes of sudden death in children, admits that a large thymus can compress the trachea, but says that in all such cases there are symptoms of gradually increasing dyspnea, the death never being sudden. He thinks that the cases of truly sudden death are due to a neurosis which influences the heart. He denies the possibility of sudden venous congestion of the thymus gland.

Targhetta,[46] in an extremely able thesis, in 1902, analyzes 41 published cases, including 1 of his own. He concludes that death occurs under different conditions and with a different mechanism in different cases. He believes in the existence of pressure, but thinks this is only an irritant to the nerves in the trachea, through which a reflex inhibition of the cardiac action results.

Howland,[26] in a valuable discussion of the subject in 1907, expresses the belief that we have no evidence of the existence of pressure, except in the rare cases in which operation has relieved the patient.

Finally, I may refer to a very interesting discussion in the Section for Diseases of Children of the Versammlung der Deutschen Naturforscher und Aerzte in 1902. Ganghofer,[18] the referee, insists on the existence of one class of cases, small in number, which exhibit dyspnea for a longer or shorter time and die of tracheal compression, not suddenly. Another class, however, dies suddenly a heart-death, not from laryngospasm. He thinks the death depends upon a constitutional disturbance independent of the status lymphaticus. Richter,[40] a co-referee,

reported 1,797 cases of sudden death in children up to the age of fifteen years. Of these, 1,555 were under one year. Only 1 case showed the status lymphaticus with an unusually large thymus. He is skeptical regarding the association of sudden death and thymus enlargement.

In the general discussion which followed, Kassowitz[28] expressed disbelief in any connection of thymic enlargement with sudden death; Ranke[35] had observed 16 cases of sudden death in the last five years, and in five of these there was no thymic enlargement present; Fischl[13] likewise spoke against the influence of the thymus in these cases; and Epstein,[9] using very forcible language, called thymus death only "eine Hinterthür für diagnostische Impotenz."

Whether or not we accept these views, the names I have quoted are sufficiently widely known to make the statements worth while considering, and to warn us against drawing a hasty conclusion, in any instance, that an enlarged thymus and a sudden death have really any etiologic relationship.

BIBLIOGRAPHY.

1. Abelin. Journ. f. Kinderh., 1870, LV., 116.
2. Baginsky. Central-Zeit. f. Kinderh., 1879, 403.
3. Barack. Inaug. Dissert., Berlin, 1894. Quoted by Rehn.
4. Beneke. Berl. klin. Wochenschr., 1894, XXXI., 216.
5. Biedert. Berl. klin. Wochenschr., 1896, XXXIII., 581.
6. Birch-Hirschfeld. Quoted by Hotz.
7. Clessin. Münch. med. Wochenschr., 1898, XLV., 330.
8. Erhhardt. Arch. f. klin. Chir., LXXVIII., 598.
9. Epstein. Münch. med. Wochenschr., 1902, XLIX., 1,728.
10. Escherich. Berl. klin. Wochenschr., 1896, XXXIII., 645.
11. Farret. Thèse de Paris, 1896. Quoted by Hotz.
12. Feer. Corresp'bl. f. Schweizer Aerzte, 1904, XXXIV., 2.
13. Fischl. Münch. med. Wochenschr., 1902, XLIX., 1,728.
14. Flügge. Vierteljahrssch. f. gerichtl. Med., 1899, XVII., 20.
15. Friedjung. Centralbl. f. d. Grewzgeb. d. Med. u. Chir., 1900, III., 465.
16. Friedlander. ARCHIVES OF PEDIATRICS, 1907, XXIV., 490.
17. Friedleben. Die Physiol. d. Thymusdrüse, 1858.
18. Ganghofer. Hünch. med. Wochenschr., 1902, XLIX., 1,728.
19. Gluck. Berl. klin. Wochenschr., 1894, XXXI., 670.
20. Grawitz. Deutsch. med. Wochenschr., 1888, XIV., 429.
21. Griffith. Transac. Assoc. Amer. Phys., 1903.
22. Hedinger. Corresp'bl. f. Schweizer Aerzte, 1904, XXXIV., 606.
23. Hedinger. Jahrb. f. Kinderheilk., 1906, LXIII., 308.
24. Hinrichs. Berl. klin. Wochenschr., 1908, XLV., 825.
25. Hotz. Beiträge. z. klin. Chir., 1907, LV., 525.
26. Howland. ARCHIVES OF PEDIATRICS, 1907, XXIV., 590.
27. Jessen. Aerztl. Sachsverständ. Zeitg., 1898, No. 21. Quoted by Hotz.
28. Kassowitz. Münch. med. Wochenschr., 1902, XLIX., 1,728.
29. König. Centralbl. f. Chir., 1897, XXIV., 605; Verhandl. d. Deutsch. Gesellsch. f. Chir., 1906, XXXV., I., 69.
30. Kopp. Denkwürdigkeiten, 1830, I.
31. Lange. Jahrb. f. Kinderheilk., 1898, XLVIII., 119.
32. Marfan. Bull. Soc. Méd. des Hôpit., 1894, XI., 361.
33. Nordmann. Corresp'bl. f. Schweizer Aerzte, 1889, 202.
34. Paltauf. Wien. klin. Wochenschr., 1889, II., 877; 1890, III., 172.
35. Penkert. Deutsch. med. Wochenschr., 1902, XXVIII., 810.
36. Plater. Observat. in Homin. affect. Pleurisque, 1614, libr. III., 172.

37. Purrocker. Münch. med. Wochenschr., 1899, XLVI., 943.
38. Ranke. Münch. med. Wochenschr., 1902, XLIX., 1,728.
39. Rehn. Verhandlung. d. Deutschen. Gesellsch. f. Chir., 1906, XXXV., II., 364.
40. Richter. Münch. med. Wochenschr., 1902, XLIX., 1,728.
41. Schwinn. Jour. Amer. Med. Assoc., 1908, L., 2,059.
42. Siegel. Berl. klin. Wochenschr., 1896, XXXIII., 887.
43. Simon and d'Oelsnitz. Bull. Soc., Pédiat., 1902, 26.
44. Somma. Archivio di Patologia Infantile, 1884. Quoted by Flügge.
45. Tailleus. Rev. mens. de la Suisse Rom., 1901, XXI., 345.
46. Targhetta. Thèse de Paris, 1902, Jan.
47. Thiemich. Vierteljahrssch. f. gerichtl. Med., 1901, XXI., 309.
48. Warthin. Osler's Modern Medicine, 1908, IV., 779.
49. Weigert. Berl. klin. Wochenschr., 1896, 887.

DISCUSSION.

DR. HOLT.—In view of the question of diagnosis of this condition by percussion, I would like Dr. Griffith to tell us if an attempt was made to map out the enlarged thymus by percussion.

DR. ABT.—Would it be possible to make Roentgen-ray pictures in these cases?

DR. SOUTHWORTH.—It seems to me that the cases which Dr. Griffith reports will probably in time be proven to be due to some disturbance of the internal secretions.

DR. KOPLIK.—These cases are very interesting in view of the fact that the tendency is to group the cases with forms of tetany. They present certain reactions clinically which ally them closely to cases of tetany. In a case which occurred in my wards recently diagnosis of tetany had been made, and the patient was believed to be doing well but died suddenly and postmortem showed very much enlarged thymus and the case is now being investigated. Escherich has recently come out with a brochure on tetany in children in which he groups all these cases of enlarged thymus as latent forms. He considers these cases of sudden death as heart failure, due to a deficiency of internal secretion, and he traces this to lesions in the parathyroid glands. Erb's phenomena with the electrical test is considered by Escherich and his school as pathognomonic of tetany. He has found in the parathyroids small hemorrhages and changes in the epithelial bodies.

DR. HAND.—I would mention a therapeutic suggestion which was given me by a general practitioner in Philadelphia who has seen several thymus deaths. He was called in to see a child with fever and dyspnea. A culture was taken and the diphtheria antitoxin given while waiting. The culture was negative, but improvement was so rapid that the doctor felt that the antitoxin must have had some good effect. He came to the conclusion that the condition was one of enlarged thymus after further study of the case. Shortly after that I saw a boy who had a history of similar attacks, and in the first attack diphtheria antitoxin had been administered. I advised a repetition of the dose, but the doctor in charge did not give it and the child died in a few hours.

DR. HOWLAND.—We sometimes lose sight of the marked con-

servatism with which this condition of status lymphaticus was first described by Paltauf twenty years ago. At that time he said that there was no explanation supported by facts to account for death, but that he believed that it was due to cardiac syncope. It seems that we have not advanced much farther than this.

Warthin thinks that as our knowledge increases, more and more instances of compression are being reported, it would seem equally probable that they are reported as rarities and that as the ordinary case of sudden death in infants and young children with thymic and lymphatic enlargement is so common no one now goes into print about it.

There seems to be ground also for the belief that enlarged thymus is a much more common condition than is usually supposed. Hochsinger made radiographic studies, and in 29 apparently normal infants he found an enlarged thymus shadow in 7. Postmortem experience with the victims of accident and only acute infectious disease points to the same thing.

Warthin says that the lymphatic cells show an exhaustion as in acute infectious diseases, such as diphtheria, and that the thymus gland is enlarged in a compensatory manner. It is difficult to understand why it should not undergo the same degenerative changes as the rest of the lymphatic apparatus. Evidence has been recently brought forward, however, to show that the thymus gland is not a lymphatic structure. It certainly is not so embryologically. Stöhr believes that it is epithelial and not lymphatic, and that histologically the cells are really epithelial cells. Chemically, in its high nucleoprotein content, as Ivor Bang has shown, it differs from lymphoid tissue. Flexner and Bunting were able to produce lymphocytotoxins by the injection of lymph nodes, spleen and bone marrow; in fact, by all lymphatic tissues. Did the thymus contain lymphoid tissue one would expect to produce similar lymphocytotoxins by its injection. I tried in vain to do this five years ago, and Moorhead, in England, was equally unsuccessful.

These embryological, histological, chemical and biological arguments constitute strong evidence against the thymus as a lymphatic organ. But we are as much in the dark as ever in regard to its function and the association of its hypertrophy with sudden death.

DR. MEARA.—It seems to me that with the accumulation of postmortem findings we have more and more evidence that the condition is but the expression of a general disturbance. A child dying last year on my service showed a thymus gland weighing nearly 9 grams. This can scarcely be called a large gland, but there was a remarkable involvement of all the lymphatic structures, splenic follicles and mesenteric glands, while the Peyer's patches and solitary follicles stood out as if carved in bas relief. This certainly was a picture of what might be called status

lymphaticus, in spite of no enlargement of the thymus, and the child died of no other apparent cause.

The way in which some of the pathologists are able to say that a certain adult cadaver will show evidences of status lymphaticus is almost weird. They base their diagnosis on the appearance of the head, which is rather square in contour, set deep between square shoulders, a peculiar anterior bowing of the thighs, due to distribution of the fleshy masses not the bone, and to the female type of pubic hair, the upper margin defined by a horizontal line. I believe the consideration of these adult types will help us in the appreciation of our problem.

DR. EATON.—I have had 3 cases of sudden death in one family. The father is a recovered tuberculous case and the mother has a bad family history. After a normal second labor she had a fairly well nourished looking baby, which on the third day died suddenly while on the nurse's lap. The next child died on the tenth day in about the same way. The last child was delivered by me two years ago; was a normal baby in every way until last June, when I was sent for one day to come to the house. I had a struggle with the child to look into its mouth, but went away from the house leaving the baby all right, but resentful of the examination. In a half hour I was sent for again and found the baby collapsed. It had acted as though it had simply fainted. I saw the case at 9:30 the next morning, when the pulse was not good, but the respiration was all right. A half hour later the baby was dead. The nurse and a younger colleague who was present said that it was evidently cardiac failure. I thought it was cardiac failure due, perhaps, to some neurosis. No autopsy was obtained.

DR. GRIFFITH (closing remarks).—I am glad to have elicited the opinion of the members of the Society on this interesting topic, and I am glad, too, that the views expressed accord so well with my own. The matter seemed to me of importance in view of the positive statements to the contrary made by recent writers.

Regarding the question of internal secretion, I think it very probably correct that the neurosis of which I have been talking depends upon defective internal secretion of some sort. There is no reason, however, to think that hyperthymization of the blood produces the condition.

The relation of rickets and tetany to this condition is very interesting. I agree with what was said by one of the speakers that lymphatism is only an expression of a constitutional disturbance just as is rickets. That is why these conditions are so often associated; the same cause that produces the one manifestation produces the other.

A PRELIMINARY REPORT OF THE METABOLISM OF DIPHTHERIA.

BY JOHN HOWLAND, M.D.,
New York.

This preliminary and as yet very incomplete study of the metabolism of diphtheria was undertaken with the desire to investigate what metabolic disturbances might be produced during the course of diphtheria and to throw some light, if possible, upon the obscure and fatal combination of symptoms usually ascribed to pneumogastric paralysis.

In a previous article I have said: "From the evidence that we have there seems reason to doubt that the vomiting, abdominal pain and slow pulse are due to degeneration of the vagus. If they are due to degeneration of this nerve we should expect to find these symptoms when degeneration has taken place and the nerve should usually be found normal when these symptoms are absent. But such is not the case. The pneumogastric nerve is found to be degenerated in practically every fatal case of diphtheria. In a series of 9 cases reported by Hibbard, 15 by Thomas, 2 by Steensland, 28 by Mallory and 14 by Thomas and Hibbard, a total of 68 in all, the pneumogastric nerve was found to be degenerated in every single case.

"These changes were found without regard to the association of symptoms. Some had slow pulse, some vomiting and some abdominal pain; others had none of these, while in many the diphtheria was very mild and the fatal outcome was referred to a complicating attack of measles or scarlet fever which supervened upon the original diphtheria infection.

"I have also found the nerve degenerated in diphtheria when there was no reason on clinical grounds to believe that it was affected.

"The vagus is also degenerated at times when bacteriologically there is no evidence of diphtheria and no clinical reason to suspect its involvement. Thomas found an extensive degeneration of the nerve in an eleven months' old child dying of pneumonia. It

seems likely then that changes in the vagus, just as changes in the sympathetic system, are exceedingly common postmortem findings.

"The slowness of the pulse, instead of the rapidity that one would expect, and the fact that the bradycardia, as Schmaltz has shown, cannot be prevented by large doses of atropin, which block the vagus effect, still further discredit pneumogastric degeneration as the casual factor."

With the thought that the vomiting, the occasional convulsions, the coma and the rapid dissolution might be due to some disturbance in chemical function which could be demonstrated in severe cases, though not the typical pneumogastric ones, I have made a number of observations upon patients severely ill and upon some less severely ill.

It is unnecessary to emphasize the difficulties in obtaining complete twenty-four hour samples of urine in a hospital where such a procedure is exceptional. Many errors are made causing a large amount of waste laboratory work. One is fortunate if 50 per cent. of the results can be utilized. Patients, too, in a contagious hospital are apt to develop complicating infections, such as scarlet fever or measles and a whole series of observations may require discarding. This has happened to me more than once. Enough analyses have been made, however, to demonstrate certain points, and some of these analyses I should like to show.

In febrile infectious processes it has long been known that there is an excessive excretion of urinary nitrogen. Two separate factors are concerned in this, the pyrexia itself and the infectious agent. It is exceedingly difficult to separate these two and to find pyrexia without infection or an infection without pyrexia. For this reason they are usually discussed together. A third factor must usually be considered; namely, undernutrition on account of the loss of appetite and digestive disturbances, or from the restriction of diet ordered by the physician. For the first few days in diphtheria this lack of sufficient food frequently occurs by reason of the difficulty in swallowing and pain caused by an extensive inflammatory angina. But it is the pyrexia and the infection that have the chief influence.

There is no strict parallelism between the height of the fever and the amount of the protein destruction, with its consequent elimination. In general, however, the excessive nitrogen excre-

tion falls with the temperature, but it may continue for several days even after a normal temperature has been reached. In certain fevers, such as pneumonia, there is an inflammatory exudate which requires absorption and the elimination of these exudates causes the so-called epicritical excretion of urea. In diphtheria, this absorption of inflammatory exudate does not occur, as almost all of it is removed by expectoration, and one potent cause for the prolonged nitrogenous excretion is removed.

As I previously said, it is difficult to separate the influence of fever from that of infection. Linser and Schmidt, from experiments on normal individuals whose temperature had been artificially raised by hot baths, believe that there is little effect upon nitrogenous metabolism until a temperature of at least 102.2° F. is reached. While this may not be entirely true, their experiments indicate that a pyrexia of less than 102° F. cannot have in itself a very pronounced effect in increasing the excretion of nitrogen.

In diphtheria we have at times exceptional opportunities to observe the influence of infection without pyrexia, for some of the patients most seriously ill may have little or no pyrexia even with extensive membrane formation. This is more true of older children and adults than of young children. On a priori grounds from our knowledge of the diphtheria toxin, its extraordinary affinity for the body cells and the difficulty of dislodging it from them, we should expect to find its chief effect manifest in those patients who have been longest without antitoxin treatment.

May I ask your attention first to the total nitrogen excretion in a few of the patients that have been studied both as to the intensity and duration of the excretion.

The intensity of the excretion is well shown in the patient Wesley, whose chart is presented. Wesley is a boy eleven years old, and very large and stout for his age. He came to the hospital, having been sick at least four days before admission. His tonsils, soft palate, uvula and posterior pharyngeal wall were covered with exudate, and he was evidently very ill. His temperature, however, was low, never above $100^{2}/_{10}$° F. In his case, therefore, any more than a very slight effect from pyrexia as a protein destroyer was eliminated. He could not take a liberal diet, and the caloric value of his food per kilogram of body weight was low, but even with this he would not have lost more than a

gram or two of nitrogen. The very great excretion of nitrogen—24 grams on the first day—is therefore exceptional and is to be referred almost entirely to the toxemia. I must confess that there is a possibility of error in this one determination, and that more than the twenty-four hours' sample may have been sent for the first examination. This I infer from the very high kreatinin nitrogen on that day, but from the manner of collection I am positive that the error could not have been large, and even if the amount of nitrogen is calculated from the average kreatinin excretion it would have still been over 16 grams with a normal temperature. Such a loss brings about a marked negative balance of nitrogen, the fecal nitrogen being considered, as is often done, as 10 per cent. of the ingested nitrogen.

It will also be seen in Wesley that the negative balance continued for a long time, even though the caloric value of his food and the total nitrogen ingested were greatly increased, almost doubled, and it was not until the fourteenth day of his illness that a positive balance was reached.

Compared with Wesley, may I show by way of contrast the results in a boy of nineteen, Craccio, who had been ill only two days before admission, but who came to the hospital with a temperature of 103°F. With him were all the three factors which produce a loss of nitrogen, high fever, toxemia and insufficient food, still more marked in his case on account of his greater caloric requirements; yet even with a temperature of between 102° and 103° F. he excreted not so much nitrogen by a third as Wesley; his minus nitrogen balance was only half as great, and, moreover, on the second day, the excretion of nitrogen fell to a constant level, and his minus nitrogen balance was perhaps no more than would be explained by his insufficient nourishment. He made, in addition, a prompt and uneventful recovery, which, as will be seen later, was not the case with Wesley.

Other instances of prolonged negative balance are to be found in the cases of Harboat, Falk and Ulmer. All three had severe local conditions untreated for four or more days. In Falk and Ulmer the temperature had been normal for three or more days before the observations were made. Harboat lost an excessive amount of nitrogen with a normal temperature, and in Falk and Ulmer the negative balance persisted for a great number of days to the nineteenth and thirteenth respectively.

It may thus be seen that during the course of diphtheria there may be a very great loss of nitrogen due to the effect of the toxemia alone and that this loss may continue far on into convalescence. These effects are also more marked when the disease is treated late.

These results are in close agreement with clinical experience. Patients, the victims of severe infection, may not have a prolonged high temperature and their local process may be rapidly and favorably affected by antitoxin and yet loss of flesh and strength and color may continue for a length of time and to a degree greater than after almost any other acute disease.

Of the cases investigated, 3 developed a marked amount of albuminuria in convalescence when the temperature had been normal for several days. As is often the case with postdiphtheritic nephritis the albuminuria, after persisting for several days rapidly disappeared. Accompanying this albuminuria there were metabolic changes most marked in the cases of Wesley and Ulmer, which may be seen on the chart. It will be noticed that in the case of Wesley the albumin increased on the eighth day from a trace to 360 milligrams, and coincident with this there was a marked rise in total nitrogen eliminated from 9.5 to 15.8 grams, which increased very greatly the minus balance of nitrogen, and which persisted for three days. The same was the case with Ulmer, though the urine for the days immediately preceding the development of nephritis was lost and the nitrogen increase is, therefore, not so apparent, but on the eleventh day of her disease the total nitrogen eliminated should have been far less than 14.5 grams.

It would, therefore, appear that coincident with the albuminuria there is a great destruction of body protein which takes place in the muscles and parenchymatous organs and probably also in the kidney itself, and the albuminuria may be only an expression of the injury done to the kidney cells, by which they are rendered less efficient for a time and allow serum albumin to pass through into the urine. The albumin was removed from the urine before the determinations were made, so that it could, of course, have no influence in increasing the amount of total nitrogen. The irritant effect of the toxin upon the kidney does not hold as an explanation at this late date, for all of the toxin has either been neutralized by the antitoxin or else fixed to the body

cells, and this would, moreover, not account for the increased excretion of nitrogen. It seems more reasonable to suppose that cells to which the toxin has been anchored early are by its influence destroyed, undergo autolysis or ferment digestion and are converted into the ordinary products fit for excretion, and that possibly similar changes in the kidney, by which the cells are rendered incompetent, allow of albuminuria.

A word should be said in regard to the excretion of kreatin. This is a substance found in muscle, and present in the urine when kreatin is contained in the food, but when the diet is free from kreatin, as in my experiments, there should be only a trace, or none, found. With the destruction of muscle that takes place in fever and cachectic processes, kreatin appears in the urine. In afebrile conditions, such as in convalescence from diphtheria, it should not be found in more than traces, and if it is found it means that breaking down of muscle is taking place. The very considerable quantity of kreatin found in Falk, Harboat, Abeles, Ulmer and Wesley, as the chart shows, indicates that destruction of striped muscle is in progress. This striped muscle may, of course, be either skeletal muscle or heart muscle, probably both, and it is interesting in this connection that Falk, Harboat and Abeles developed well-marked myocarditis and Ulmer and Wesley a moderate degree of this condition.

So far as the distribution of undetermined and ammonia nitrogen was concerned, there was nothing decidedly abnormal. It has been shown by numerous observers in many febrile conditions, and even in poisoning by substances producing most marked pathologic changes in the liver, that the nitrogen partition may be within normal limits. This is merely another indication that the synthesis of urea may not be interfered with, even though a severe toxemia may be present.

Up to the present time, I have not been able to investigate an uncomplicated fatal case or one showing persistent vomiting, pain and slow pulse. But enough has been done to make it apparent that there is a marked and prolonged excretion of nitrogen evidently due to the toxemia; that with the development of albuminuria there is evidence of a great destruction of body protein, and that destruction of striped muscle takes place even with a normal temperature, as shown by the excessive kreatin excretion.

TABLE I.

POSTDIPHTHERITIC ALBUMINURIA.

Name	Day	Food N	Total N	Albumin N	Balance
Wesley	6	8.4	13.	0	—5.3
"	7	8.4	9.5	trace	—1.8
"	8	8.4	15.8	0.36	—8.2
"	9	8.4	11.	0.15	—3.4
"	10	8.4	12.7	0.25	—5.1
"	11	10.5	0.6	0.2	—0.2
Ulmer	11	6.	14.5	0.6	—9.1
"	12	7.7	11.2	0.15	—4.25
"	13	7.2	8.3	trace	—1.75

TABLE II.

NITROGENOUS EXCRETION.

Name	Wt. in kg.	Food N	Cal. per kg.	Urinary Nitrogen				N balance	Day
				Total N	Kreatinin	Kreatin	N as alb.		
Wesley	51	8.4	21	23.9	0.57	0.17	0	—16.3	5
"	"	8.4	21	13.	0.32	0.18	0	— 5.3	6
"	"	8.4	21	9.5	0.30	0.16	trace	— 1.8	7
"	"	8.4	21	15.8	0.39	0.33	0.36	— 8.2	8
"	"	8.4	21	10.95	0.30	0.20	0.15	— 3.4	9
"	"	8.4	21	12.7	0.33	0.27	0.25	— 5.1	10
"	"	10.5	31	9.6	0.26	0.11	0.2	— 0.2	11
"	"	11.6	34	11.75	0.31	0.13	trace	— 1.3	12
"	"	11.4	35	10.55	0.30	0.10	0	— .61	13
"	"	11.9	40.5	8.25	—	—	0	— 2.5	14
Craccio	60	8.4	18	16.35	0.85	0.12	0	— 8.7	3
"	"	8.4	18	10.64	0.42	0.02	0	— 3.	4
"	"	8.4	18	10.75	—	—	0	— 3.	6
Harboat	55	9.	20	24.14	0.43	0.33	0	—16.	7
Falk	63	8.4	17	15.12	0.02	0.06	0	— 7.6	14
"	"	5.6	16	13.74	0.47	0.15	0	— 8.7	15
"	"	10.	28	12.90	0.50	0.22	0	— 3.9	19
Ulmer	54	6.	14	14.5	0.53	0.18	0.6	— 9.1	11
"	"	7.8	18	11.15	0.36	0.13	0.15	— 4.25	12
"	"	7.2	17	8.26	0.32	0.09	trace	— 1.75	13

TABLE III.

EXCRETIN OF KREATIN.

Name	Day	Temperature	Kreatin N
Falk	13	normal	0.15
"	15	"	0.15
"	19	"	0.22
Harboat	7	"	0.33
Abeles	11	"	0.17
Ulmer	11	"	0.18
"	12	"	0.13
Wesley	5	100	0.17
"	6	99	0.18
"	7	normal	0.16
"	8	"	0.33
"	9	"	0.20
"	10	"	0.27
"	11	"	0.11

DISCUSSION.

DR. MEARA.—It is somewhat hazardous for a general practitioner who has left his physiological chemistry fifteen years behind him to approach a problem of metabolism based on the modern methods and technique. However, this work was not done as a source of amusement to the chemist, but as a contribution to medical science; and it is incumbent on us to deduce the practical lessons conveyed in these figures.

A good deal of work has been done to show the proteid breakdown in fever both in animals in whom pyrexia was established by the injection of organisms by subjection to heat in baths, by brain puncture, and in man by the reaction to tuberculin, by the hot bath and, in a few instances, in the course of familiar fevers.

In most of these cases the ordinary nitrogen partition was done, which showed by the large negative nitrogen balance that the body was losing far more nitrogen than it took in; but these older figures gave us little information as to the source of the loss. Some half dozen years ago Folin added new significance to the nitrogen partition by calling attention to kreatinin, which had heretofore been looked upon as a nitrogenous by-product, which, like Agassiz's snail, was to be trodden under foot because it did not fit into the classification.

Folin showed how constant was the output of kreatinin in any

one individual, in the presence of great variations in nitrogen intake and output in that individual, which you see shows beautifully in Dr. Howland's figures. It was looked upon as an expression of muscle katabolism, and it might be expected to increase in the presence of such destruction of tissue as we have at hand; but you see in these figures it remains constant. Later Shaffer called attention to kreatin, a substance not normally present, except in minute quantities, in the urine when a kreatin-free diet is given. This substance does vary, as you see, with the nitrogenous waste, and is to be looked upon as the real expression of muscle breakdown, while the kreatinin is to be considered as the product of some special function of muscle action, as muscle tonus. So you see that while the negative nitrogen balance shows us that tissue has been destroyed, this last column of figures, kreatin, tells us what tissue.

If you add up the nitrogen loss in Wesley's case, throwing out the first figure as possibly doubtful, it amounts to about 30 grams, which represents a loss of muscle substance equal to 1 kilo, *i.e.*, 2 pounds, of not body weight, mind you, but muscle weight, and shows us what a rapid and tremendous loss is going on.

Now, let me call your attention to the albuminuria. Notice how the loss of nitrogen increases on its appearance. To my mind that means, not that we have an irritation of the kidneys, a cloudy swelling, an acute parenchymatous nephritis, but that, taken with the kreatin output, the kidney is undergoing the same changes as other tissues, that is, an autolysis. This heightens the significance of postfebrile albuminuria, and what still more enhances it is that in each case there was a concomitant myocarditis. We all know how difficult at times the diagnosis of myocarditis is; hence, albuminuria is a warning to watch the heart closely, or, even in absence of physical signs, to exercise prophylactic measures, for the heart is probably undergoing the same autolytic changes as occur elsewhere.

Finally, note the effects of an increased diet on this nitrogen waste. You will note that Wesley was on an intake of 8 grams of nitrogen, which is enough to establish a nitrogen equilibrium on, and even more, as Chittenden has shown. But you see the caloric content of his food was only 21 per kilo. Now a boy of eleven years, up and about, needs 40 to 60 calories per kilo, and while less in bed, still a man in bed, trained to the greatest degree of muscular relaxation, needs 22, and ordinarily 26 to 28, while a boy in bed is in a condition of more or less muscular activity. You see that when Wesley's food was carried up to 10 to 11 grams of nitrogen and 38 to 40 calories, the nitrogen loss diminished, and though you might say that this was concomitant with the cessation of the process, still Shaffer's work on typhoid fever has demonstrated the sheltering effect of sufficiently high caloric values in food. These patients of Shaffer's, I understand,

not only lost less flesh, but suffered from fewer nervous symptoms and convalesced more rapidly than the average. Our typhoid patients have been starved, and not only suffered from the toxemia of typhoid, but also from inanition and the results of the necessity of tearing the nitrogen out of their own tissues to supply themselves with the carbohydrate moiety of the proteid molecule.

I do not believe that if the gentlemen present were to look over the histories of the typhoid cases in their services, 1 per cent. would be found sufficiently fed. You will find the milk diet ordinarily given to contain 800 to 900 or 1,000 calories, when 2,500, 3,000, and even more, are needed, for you must remember that a man in fever uses as many calories of food in his daily metabolism as a man in health when not active, and perhaps more. In Dr. Howland's cases the difficulty arose with the sore throats that prevented more ingestion, but that does not obtain for many other febrile conditions.

This paper teaches us, then, what destruction of tissue goes on in diphtheria, the meaning and importance of postfebrile nephritis, the probability of myocarditic involvement and how much sufficient feeding means to the patient.

I think the paper a very valuable contribution to our knowledge.

PNEUMOCOCCIC INFECTION.

BY WALTER LESTER CARR, M.D.,
New York.

Since the pneumococcus was first described (Fränkel and Weichselbaum) its connection with pneumonia has been accepted as the etiologic factor in a majority of cases of this disease, but it is not always remembered that it is found in pleurisy, pericarditis, endocarditis, meningitis, otitis, diseases of the accessory sinuses of the nose, arthritis, osteomyelitis, diseases of the liver, spleen, kidneys, lymph nodes, skin, and mucous membrane. It may be present in bronchitis without pneumonia, or in otitis without any subsequent inflammation of the lung. Infections of the ethmoid and sphenoid structures may be observed in patients who have no evidence of lung disease, and peritonitis may occur independent of pneumonia.

Park and Williams showed (*Journal of Experimental Medicine,* Vol. II., 1905) that typical pneumococci were present during the winter months in the throat secretions of a large percentage of healthy individuals in city and country. Under these conditions they are not, however, always pathogenic for animals; in many instances they are virulent in only from 15 to 20 per cent. of such cases. The virulence of the organism is variable; in the sputum of a case of lobar pneumonia early in the disease it is likely to be very virulent. The pneumococcus can be recovered from the blood in practically all cases of croupous pneumonia and other infections.

Desguin, in an article (*La Septicémie Pneumococcique*, Bruxelles, 1908) states that the pneumococcus is becoming daily more important in both medicine and surgery. This is chiefly shown in the fact that much so-called grip and grippal disease in general is caused by the pneumococcus. The pneumococcus is closely related to the streptococcus, having all the qualities of polymorphism and ability to cause diseases of widely dissimilar types.

The entrance of this organism into the deeper tissues when a surface lesion occurs can be readily understood. Its action will, of course, be favored by any condition of depressed vitality.

Netter gives (Muir and Ritchie, *Pathology*, 1907) the fol-

lowing tables of the relative frequency of the primary infections by the pneumococcus in man:—

(1) In adults: Pneumonia, 65.95 per cent.; bronchopneumonia, capillary bronchitis, 15.85 per cent.; meningitis, 13 per cent.; empyema, 8.53 per cent.; otitis, 2.44 per cent.; endocarditis, 1.22 per cent.; liver abscess, 1.22 per cent.

(2) In children, 46 cases were investigated. In 29 the primary affection was otitis media; in 12, bronchopneumonia; in 2, meningitis; in 1, pneumonia; in 1, pleurisy; in 1, pericarditis.

Pneumonia.—The diplococcus pneumoniæ is found, according to Weichselbaum, in 70 per cent. of cases of croupous pneumonia, but Park states that it is present in 95 per cent. Charazain-Witzel found the coccus restricted to the lung in 66 per cent. of cases of pneumococcus infection. Others have found a much higher percentage of blood infection. Eichhorst found the pneumococcus in the blood in all the cases he studied.

Pleurisy.—The pneumococcus is the common factor in fibrous pleurisy (Lord, in *Osler's Modern Medicine*). It is only rarely a cause of clear serous effusions, but is more common in the primary empyemas of children, and in all effusions complicating lobar pneumonia.

Empyema.—The pneumococcus appears to be the most frequent organism occurring in empyema as a pure infection. Of 109 empyemas investigated by Netter, it was present in 53.6 per cent. of 28 cases in children; while in 89 cases in adults it was found in 54 of 137 cases, or 39.4 per cent.

In pleurisies the pneumococcus shows a marked tendency to die out; it may be found in smears, but cultures are frequently sterile, which shows why these cases run a favorable course.

When the pneumococcus invades the pleura it produces the same constitutional symptoms as if the lungs were involved. In tuberculous empyema associated pneumococci may cause suppuration.

Ear.—Muir and Ritchie (*Manual of Bacteriology*, 1907) state that the primary cause of infection in a great many cases is an otitis media, such infection being from the nasal cavities.

Yearsley (*Text Book of Diseases of the Ear*) states that Hasslauer (*Klinische Beitrage*, Vol. V., No. 3) examined 82 cases microscopically by cultures and by animal experiments. Of 63 primary cases of otitis, 42 were examined after paracentesis, and 19 after spontaneous perforation. In both groups

the diplococcus of pneumonia was found most frequently, the staphylococcus coming next.

Meningitis.—Elter (*Journal of Medical Research*, Vol. XIV.) found the pneumococcus in 2 of 102 cases of meningitis.

L. Kirchheim (*Med. Klinik.*, 1908, Vol. IV., p. 1,460) (*Meningismus und Meningo-Encephalites bei Kroupöser Pneumoniae*) had, among 500 cases of pneumonia, 6 with meningeal symptoms and clear cerebrospinal fluid, the symptoms appearing very early in the disease. He calls the symptoms meningism. Three other cases he calls meningoencephalitis. The symptoms, which may appear early, late or only after the crisis, are not only meningeal, but indicate focal cerebral lesions as well and may last for some time—ten and one-half weeks in one case. At autopsy, hyperemia and edema of brain and meninges, with accumulation of clear fluid, were found. Microscopically, there was round-cell infiltration around the vessels and even at some distance from them in the gray matter of the brain and cord. This pneumococcic meningitis was always fatal. The number of cases recorded is not given.

In many instances where no other lesions are present the extension is by the Eustachian tube to the middle ear. In other cases the path of infection is from some other lesion by means of the blood stream. A considerable number of cases, especially in children, are due to the pneumococcus alone, but the pneumococcus may be associated with tubercle bacilli here as elsewhere. Desguin reports a case of pneumococcic abscess of the brain in a child aged four years. The germ was doubtless conveyed through a penetrating fracture of the skull. The lesions remained localized. In another child of thirteen there was traumatic pneumococcic meningitis. There had been a fracture of the base of the skull, setting up otitis and meningitis. The lesions were all intracranial.

In a recent case under my observation a baby, aged one year, was admitted to the New York Eye and Ear Infirmary, on the service of Dr. Marple, shortly after recovering from pneumonia, with an acute conjunctivitis. Pneumococci and staphylococci were found by Dr. Waters, the house surgeon, in smears from the eyes. The child had scorbutic symptoms, but there were changes in the ankle joints independent of the scurvy, and these were regarded as due to the primary infection. All tuberculin tests were negative. Two months later the child developed

meningitis. Lumbar puncture showed pneumococci. Death from meningitis ensued nine days after initial chill.

The fluid drawn by lumbar puncture may be, as stated by Kirchheim, clear in cases of meningism (pneumonia with cerebral symptoms), or it may be turbid or purulent in meningitis from the pneumococcus. This purulent character is mentioned by a number of authors, Allard being the latest authority. (*"Lumbal Punction," Ergebrisse der Innere Med.,* 1909, p. 125.)

Pericarditis.—Direct extension of inflammation is probably very rare. In pleuropneumonia, in which it is said to occur, the invasion of the pericardium is not an extension of the inflammation from the pleura, but an infection of the pericardium by pneumococci brought from the lung—generally by the blood, but sometimes by the lymphatic circulation. Some believe that the pericardium is more often infected at the same time and from the same source as the lung, and not secondarily to it.

Preble believes (*American Medicine,* June 15, 1901) the liability to pericarditis in unilateral pneumonia to be in the ratio of 1 to 40 cases; in bilobar and trilobar, 1 in 10; and in quadrilobar, 1 in 5. It is more common in affections of the right lung than of the left.

Holt states that pericarditis is rare, and in his series of pneumonia cases he saw only 2 with pericarditis associated with inflammation of the left lung.

Endocarditis.—In an editorial note in the American edition of *Nothnagel's Practice,* it is recorded that Wells found endocarditis in 3 per cent. of all cases, or 4 per cent. of all fatal cases of pneumonia. But in 517 postmortem reports it was noted in 20 per cent. Pneumococcic endocarditis may occur, of course, without present or past pneumonia. Muir and Ritchie (q. v.) state that in some cases of ulcerative endocarditis following pneumonia the pneumococcus is present.

Rosenow, in a recent contribution (*The Journal of Infectious Diseases,* April, 1909, Vol. VI., No. 2), has made a careful study of a number of cases of endocarditis. Some of these were protracted, and particular interest attaches to them because of the blood cultuures that were made as to the cause of inflammation. The pneumococcus was present either as a primary cause of endocardial disease or as a secondary infection of old processes of supposedly rheumatic origin. Endocarditis was developed in 5

of 10 animals inoculated before the special characteristics of the pneumococci were lost. Pericarditis developed in 6 cases. In 2 endocardial lesions were absent, while pericarditis developed and also beginning thrombosis of the portal vein. In a clinical group it was observed that 2 cases followed tonsillitis.

The exact duration of the disease could not be accurately determined, but probably ranged from four to twelve months or more. The blood cultures were the means of making the diagnosis in almost all cases. In some cases it was difficult to make a correct diagnosis because of the insidious onset and chronicity, and most of all because the acute process was ingrafted upon an old valvular lesion.

Wadsworth (*Medical Record,* December 28, 1907) states that experimental studies have demonstrated the importance of previous injury of the endocardium in determining the secondary localization of the infection in the heart. He makes mention of the fact that while the infection of the lung by the pneumococcus is almost always benign, it is in the heart practically fatal.

Peritonitis.—The diplococcus pneumoniæ was several times found to be the only microörganism, especially in the acute peritonitis of infants. (*Nothnagel's Practice. Diseases of the Intestine and Peritoneum.* Philadelphia, 1904.)

From its higher incidence in girls, and from the fact that the inflammation is frequently limited to the lower part of the abdomen, so that it often simulates appendicitis, Brun thought that the pneumococcus might find an entrance through the Fallopian tubes.

In 33 cases collected by Quéhart and given by Nothnagel, 27 were in girls and 6 in boys; it is rare in men, but is less frequent in men than in women.

According to Dieulafoy, the onset of this form is sudden, without prodromal symptoms; there are sudden pain, vomiting, and subsequent abdominal symptoms.

Clarke (*Bristol Medico-Chirurgical Journal,* 1907, Vol. XXV.) observed 2 cases of peritonitis of unusual interest:—

In the first case, a girl of nine years, the abdominal symptoms were first noted and there was a spontaneous opening. The pus showed the pneumococcus in pure culture. In this case the dullness was over the front of the abdomen. The appearance of the child indicated tuberculosis. Pneumonia occurred one month after the abdominal symptoms.

The second case was in a girl of four, sister of the above, and an incision gave pus that showed the pneumococcus in pure culture.

Marfan states that before puberty the tender area of pneumonia is felt over the abdomen. The fact that tenderness sometimes exists in such cases before the diagnosis of a pulmonary lesion is apparent is used by this author to explain that the coccus has primarily attacked some nearby organ (appendix, spleen, stomach, etc.) before invading the lung. The conditions are sometimes reversed, as when a peritonitis leads at first to the diagnosis of a pneumonia.

Many cases of pneumococcic disease are located in the digestive tract. It is probable that many visceral infections occur by direct absorption through the digestive tube. Marfan reports a case in a child, aged four, with the following localization of pneumococcic infection: Appendicitis, ulcerous enteritis with multiple perforations, and general peritonitis. The child recovered, treatment consisting largely of serotherapy.

Another child, aged nine, had appendicitis of the influenza type, encysted peritonitis and intestinal perforations. Recovery occurred under serotherapy.

Arthritis.—Herrick, in an article on "Pneumococcic Arthritis" (*American Journal of the Medical Sciences*, 1902), reviews the subject and refers frequently to Leraux (Paris, 1899) and Cave (London, 1901). Many authors do not mention this form of arthritis, and it probably occurs once in 800 cases of pneumonia. Of 47 cases, 40 were males and 7 females. Before ten years there were 4 cases, but between ten and twenty years there were no cases. The other cases reported by Herrick occurred after twenty years of age. Two cases, 1 a child, were also observed, in which no cultures were made. Recovery followed.

The knee is the joint most often affected. Holt had 1 case in which the shoulder joint was affected. Pfisterer (*Jahrb. für Kinderhk.*, April, 1902) gives a mortality of 65 per cent. in the joint cases.

Complications.—Clarke (*Bristol Medico-Chirurgical Journal*, 1907, Vol. XXV.). Cases illustrating the more unusual complication of pneumonia. In 126 cases there were: Peritonitis, 2 cases; thrombosis of vessels, 3 cases; endocarditis, 2 cases; nephritis, 1 case; arthritis, 1 case, besides empyema, etc.

Thrombosis.—Of 32 cases collected by Steiner (*Johns Hop-*

kins Bulletin, 1902), 27 occurred during convalescence, and, according to Osler, thrombus nearly always occurs in the femoral veins. It is more uncommon in the arteries than in the veins.

Thrombosis of the cerebral vessels occurs either early in the disease or during early convalescence. A general thrombosis may result from the systemic infection. One fatal case is recorded by Clarke.

Pic and Bonnamour (*Archiv. Gén. de Méd.,* 1908, p. 593. *Formes Cliniques de la Septicemie Pneumococcique*) from a scientific point of view would divide all pneumococcic infections as follows:—

I. Localized.
   (*a*) In the form of pulmonary congestion.
   (*b*) In the form of pneumonia.

II. Generalized.
   (*a*) Following pneumonia.
   (*b*) Following pulmonary congestion.
   (*c*) Terminating in pneumonia.
   (*d*) After penetration of the pneumococcus through the skin.

Clinically, there are four classes:—

1. General and local phenomena equally attenuated.
   (*a*) Different forms of congestion of Woillez.
   (*b*) Splenic phenomena of Grancher.
   (*c*) Pleural effusion of Dieulafoy.

2. General and local phenomena intense; lobar pneumonia acute.

3. Local phenomena manifest, but the general ones are aggravated and mask the local ones.
   (*a*) Extrapulmonary localization of the pneumococcus.
   (*b*) General infection.

4. Local phenomena absent; general ones predominate or are present alone.

Free primary septicemia without initial phenomena. All reported cases (4) have thus far terminated fatally.

The infection by the pneumococcus should be recognized in the different organs and tissues, and blood cultures should be made in all doubtful cases.

68 West Fifty-first Street.

REPORT OF A CASE OF LONG-STANDING EMPYEMA WITH A NOTE ON RACE AS A PREDISPOSING FACTOR IN THE OCCURRENCE OF EMPYEMA.

BY ALFRED HAND, JR., M.D.,

Philadelphia.

It is difficult to determine from the history of this case the length of time during which the purulent pleural effusion existed, but my own belief is that it had probably been present for a year and a half, or perhaps even twenty-two months.

The patient, a girl, was three years and five months old, and weighed eight pounds and fourteen ounces when admitted to the Children's Hospital. Nothing could be learned of her parentage; at six months of age she was taken from a city institution to the country and placed in a private family under favorable surroundings. The only point ascertainable about the feeding was that she was at first fed on a mixture of cows' milk and barley-water. She never had any contagious disease, but when fourteen months old there was a report (to the institution from which she went to the country) that she "still had bowel complaint." Subsequent reports were:—

At eighteen months; has been very sick but is better; one week later, again very sick.

At nineteen months; has always been a very delicate baby; has a wasting disease with little hope of recovery. Two weeks later, some improvement is shown and the physician now thinks there is a possibility of recovery.

At twenty-one months; has been very sick again; the physician is surprised that she has lived so long and thinks her parents must have had consumption.

At twenty-three months; is a marasmus baby and is very sick.

At twenty-five months; is very delicate.

At thirty-two months; the physician thinks she has gained slightly but she is still very delicate.

When admitted to the hospital the statement was made that she had a cough which had lasted for at least eighteen months, and that she sometimes vomited in the attacks of coughing. The bowels were usually constipated and prolapse of the rectum was of frequent occurrence.

The accompanying photograph shows her condition as well as a verbal description, but neither can convey the uncanny sight presented by the child when stripped for examination. The most striking features, at first glance, were the absolute emaciation and the distention of the abdomen. The latter suggested an abdominal

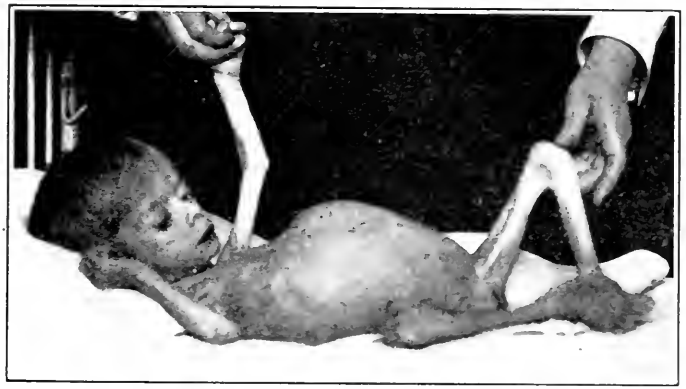


FIG. I. M. C., 3½ years old, weight 8 lbs. 14 oz., showing distention of abdomen and depression of left chest.

growth or hypertrophic dilation of the colon, but both of these could be definitely excluded on examination. As the child moved the flail-like arms over the trunk, with the hands which looked like claws scratching the skin, it required an effort to realize that the patient was a human being, and the sight was one from which many turned away with a shudder. The photographs show well the difficulties encountered in a physical examination of the chest and are our only excuse for not detecting the empyema immediately on admission.

From the notes made when the child was admitted the following are extracts:—Head is frightful to behold; eyes are sunken; ears are prominent. Fontanel is closed. Teeth, eighteen in number, are irregular and poor. Tongue clean, throat clear.

Chest is much deformed on the left, the ribs in the cardiac region being depressed while the lower ones flare out. There is a marked lateral curvature of the spine, with the concavity toward the left (Fig. II.) which does not straighten out on lifting the child by the shoulders. Over the lower part of the back the skin has some areas which are perfectly white, varying in size up to 1 cm. in diameter. Over the sacrum there is considerable long hair and below this are some scars of recently healed sores.

FIG. II. Same as Fig. I., but showing lateral curvature of spine, contraction of left side, white area of skin at point marked X, and extreme flexion of left forearm on arm.

The examination of the lungs was difficult because of the diminutiveness of the thorax, the emaciation and the crying, which occurred whenever the child was examined; but the right lung seemed perfectly clear and the cry seemed to be transmitted well over the left side.

The apex beat of the heart was in the third interspace in the midclavicular line and the heart-sounds were normal.

The walls of the abdomen were so thin that the coils of intestine were plainly visible. The liver and spleen were not palpable and no masses could be detected beneath the relaxed abdominal muscles.

A few black and blue spots were present on the chest, epigastrium and right thigh. Other similar spots appeared in many parts of the body from the traumatism of even the most gentle handling; thus, drawing the cheeks forward so that the milk would not run out of the mouth caused ecchymoses where the nurse's fingers rested.

The mental condition was that of backwardness; the child was unable to talk but the expression was not that of an idiot.

Three days after admission the weight had increased one pound, which proved to be the maximum attained, most of the increase being due to undigested milk lying in the intestines; fifty-six ounces being taken in twenty-four hours. This amount

had to be reduced, as the digestive organs could not handle it, great abdominal distention following. The weight then fell to nine pounds, four ounces, after which there was a slow gain again up to nine pounds, fourteen ounces, with a slight loss after she was transferred to the surgical wards.

Some of the measurements were:—Length over all 70 cm. (27.5 inches). Length of foot, 10 cm. (4 inches). Fronto-occipital circumference, 42 cm. (16.5 inches). Girth of abdomen at umbilicus, 42 cm. Girth of abdomen midway between umbilicus and ensiform, 46 cm. Circumference of middle of upper arm, 5.5 cm. (2⅛ inches). Circumference of forearm in largest part, 7 cm. (2¾ inches). Circumference of calf 6.5 cm. Circumference of thigh, middle, 7.25 cm. The urine was negative except for a faint trace of albumin. The blood-count gave 55 per cent. hemoglobin, 4,780,000 red cells and 15,000 leukocytes.

After some days of study, we at length became so accustomed to the weird sight that we were able to recognize that the right chest measured 22 cm. and the left only 20 cm., that over the latter the breath-sounds were absent and the percussion-note was dull, almost flat, and that the right border of the heart was distinctly to the right of the sternum. Through an exploring needle inserted in the third interspace in the left anterior axillary line, thick green pus was withdrawn containing streptococci; examination for tubercle bacilli was negative. The needle of an aspirator was then inserted and one ounce was obtained, but as the flow then presented a tinge of blood the suction was shut off. When the needle was pushed in, there was a grating sensation as though it was passing through either a very dense or a calcareous pleura.

On the following day, my surgical colleague, Dr. Jopson, resected an inch of the seventh rib, under cocain-infiltration-anesthesia, finding a dense fibrous pleura about a quarter of a inch thick; about an ounce of green pus was obtained, the child standing the operation very well. The wound discharged properly, but showed no tendency to heal and one month later, in a Philadelphia hot wave, the child succumbed.

An autopsy revealed nothing beyond the compressed, airless, fibroid left lung, with the greatly thickened pleura. All of the other organs were normal, with the exception of a relative, moderate dilation of the stomach and intestines.

The above is an extreme and unusual illustration of the result of overlooking an empyema, and may help to emphasize the

importance of recognizing the condition at the earliest possible moment. Without discussing at the present time the diagnosis of empyema, I may say that one of the most suggestive points is a temperature which remains moderately elevated (100°F.) after the crisis of a pneumonia. Two years ago, while examining a colored child whose temperature was behaving in this way, the thought suddenly occurred to me that I had never seen an empyema in a negro. Since then watch has been kept with special reference to this point, and the records of the Children's Hospital have been consulted for the past ten years. My thanks are due to my colleagues on the staff for the courtesy of allowing the use of their cases, and also to Dr. J. K. Walker for his valuable aid in collecting the figures. In that time there have been 1,109 cases of pneumonia in white children with 99 empyemas, or 9 per cent. There have also been 245 cases of pneumonia in colored children with 3 cases of empyema, or 1.2 per cent.

A study of these 3 cases would justify the conclusion that only 1 could reasonably be classed as the metapneumonic form of empyema. The first one, occurring in an eleven-months-old infant, gave 2 drams of pus containing diplococci, the autopsy showing a pleuropneumonia. The second one gave signs of bronchopneumonia, with a pleural effusion on one side; in a hard coughing spell a large amount of pus was expectorated and signs of a pyopneumothorax developed; death occurred in a few days and the autopsy showed a perforation in the bronchus with pus and air in the pleural cavity; there was also a widespread general tuberculosis, and it is very likely that the effusion was primarily serous, perhaps becoming infected on the development of the pneumothorax. The third case had a large purulent effusion, which gave a pure culture of the typhoid bacillus. If we exclude these 2 cases their places might be filled by 2 cases which have occurred in my service this year and were of the regular metapneumonic type, diplococcal in origin. Even if all are admitted there is still a wide difference in the susceptibility of the white and colored races to empyema. An interesting point is raised by the fact that the 2 which occurred this year were in mulattoes, not in full-blooded negroes. The number of cases is, of course, too small to decide whether, in the admixture of the races, the susceptibility of the white or the immunity of the negro will triumph.

DISCUSSION.

DR. FREEMAN.—In regard to the operation I would like to say that in my experience at the Foundling Hospital resection of the rib in these cases is absolutely unnecessary. It produces considerable shock and causes deformity of the chest. Autopsies have shown that a tube inserted between the ribs provides efficient drainage.

DR. ADAMS.—In regard to the difficulty of diagnosis, some years ago I had a case which presented all the symptoms, even obliteration of the intercostal spaces, and in the presence of the staff I introduced the needle and got pus. The child was handed over to the surgeon, who did not find any pus in the pleural cavity. The child died in a couple of days and postmortem showed an abscess at the spot where the needle had gone in.

This winter I saw 2 cases—one in private practice in which the needle was introduced and with the syringe pus withdrawn. He was turned over to the surgeon, who failed to find anything in the pleural sac. The child finally died of abscess of the lung.

About 40 or 50 per cent. of cases in our hospital are negroes and there are a large number of cases of pneumonia, and yet I could not recall a case of typical empyema in a negro which accords with Dr. Hand's experience, but a case occurred recently with a history of empyema. I took the precaution to state that a thickened pleura would sometimes obscure things and that I was not quite sure. A student, under my direction, introduced the needle and the pus came out promptly. I remarked that the lung must be very much retracted, as he had gone in the length of the needle. The case was turned over to the surgeon, and not a drop of pus found in the pleural sac. He had gone into an abscess. In none of these three instances did any harm result, but the diagnosis in these cases is very difficult.

DR. ROTCH.—Non-tubercular empyemas almost invariably get well. They may break into the abdomen or into a bronchus, but if properly treated surgically they get well in almost every instance. We still resect the rib in our cases and do not get much malformation.

DR. HAND (in closing) agreed with Dr. Freeman as to resection of the rib. He preferred simply to put in a drainage tube.

A CASE OF CONGENITAL STENOSIS OF THE DUODENUM.

BY ROWLAND G. FREEMAN, M.D.,

New York.

Congenital obstruction of the duodenum, although very rare, is occasionally found, and inasmuch as the symptoms produced are quite similar to those found with congenital hypertrophic stenosis of the pylorus, the possibility of such a lesion should be borne in mind when these symptoms of obstruction in this portion of the alimentary tract occur.

Congenital obstruction may occur in various parts of the intestine and in one of the recent collections of cases—that of Kuliga†—of a total of 185 cases of occlusion of the intestines, 45 were found in the duodenum.

Of 57 cases of occlusion of the duodenum, collected by Cordes,‡ 48 she classes as atresia and 9 as stenosis.

There is, however, a third class of congenital obstruction of the duodenum which neither forms a complete obstruction termed atresia or an almost complete obstruction termed stenosis, but through folds of the mucous membrane causes a valve-like obstruction. One case of this sort has been reported by Preisich,§ where there was a double valvular occlusion. He quotes 3 other cases of similar occlusion.

Besides the 57 cases recorded by Cordes, others have been reported more recently by Schukowski,¶ one by Taillens,‖ that of Preisich, and the case reported before this Society by Drs. Shaw and Baldorf.** These make, with the case that I am to-day reporting, 63 cases.

The cause of this condition is unknown. Etiological factors

† Beitrage sur Pathol. Anatomie., 1903, p. 481.

‡ ARCHIVES OF PEDIATRICS, June, 1901.

§ Jahr. für Kind., 1903.

¶ Med. Rundschau, 1903.

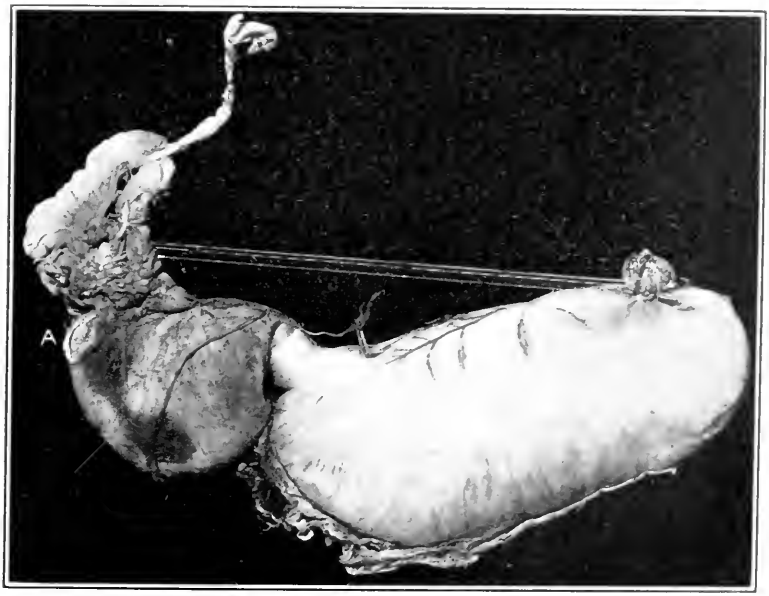
‖ Rev. Med. de la Suisse, Vom. XX., March, 1903.

** ARCHIVES OF PEDIATRICS, 1907, p. 813.

that have been mentioned are syphilis and pressure by the head of the pancreas. It is also noteworthy that the position of this occlusion is also the position of an occasional diverticulum and that the position of the Merkel's diverticulum is also the position of an occasional congenital occlusion.

I am indebted for this specimen, the history and the autopsy notes to Dr. W. Morgan Hartshorn, with whom I saw the case in consultation.

The male child, from whom this specimen was obtained, was



STOMACH AND DUODENUM OF A CASE OF CONGENITAL STENOSIS OF THE DUODENUM.
At A the duodenum is cut open to show the point of stenosis.

born of a primipara, who had gone through pregnancy without symptoms, and had had a short, normal labor. The child was asphyxiated at birth, but soon appeared normal.

For twenty-four hours the baby showed no symptoms, but on the second day after nursing it was noted that no urine had been passed, and the baby vomited matter of a dark brown color and fluid, but after stomach and colon washing the vomiting was apparently controlled. On the third day there was no vomiting. Meconium was passed from the bowels, and the baby became

markedly jaundiced. On the fourth day, as the baby would not take the breast and seemed very weak, it was fed breast milk, an ounce at a time, as well as a modification of cow's milk by gavage. At this time the vomiting was continuous and projectile.

On the fifth day frequent vomiting continued, the jaundice was marked, the stomach was washed and the child was fed by gavage.

On the sixth day when I saw the child it was very emaciated, markedly jaundiced, very weak and refused to nurse. No peristalsis could be discovered. A light yellow material in some of the movements seemed to indicate that some milk had passed into the lower bowel. The child did not seem in a fit condition for operation. The following day it was better, and nursed a little. On the eighth day also the child seemed to be doing fairly well. On the ninth day, however, the vomiting became worse, and on the tenth day the child died. The temperature was never above 99°.

Autopsy.—Markedly jaundiced. Weight, about 6 pounds. Anterior fontanel large. Several hemorrhagic areas on face and body. Umbilical cord partially separated. On opening the abdomen, the stomach presented itself, showing apparently two dilated portions with a small constriction two-thirds of the distance toward the pylorus. This constriction was shown later to be the pylorus, the second dilated portion to be the dilated duodenum above the stricture. Air could be forced through the pylorus from the larger to the smaller cavity. The duodenum near the constriction was firmly bound down by adhesions, which were deeply bile-stained.

The stomach capacity was about three times that of the dilated duodenal cavity. The walls of the stomach were much thicker than the dilated duodenum.

The pylorus was normal and patent. The stenosis of the duodenum was complete, and surrounding this was a large amount of connective tissue. The point indicated by A in the illustration is the point of stricture.

The stomach measured 4½ inches in length, while the dilated duodenum measured 2 inches in length. Sections of the wall of the stomach and duodenum were examined and showed nothing abnormal.

DISCUSSION.

DR. ROTCH.—I think what will be extremely interesting in the future will be the carrying out of investigations of these cases with bismuth. The cases are fairly rare, but the cases of young babies where symptoms of stricture occur are not so infrequent. It is especially important to continue such work as this in regard to making the diagnosis and in connection with volvulus and twists of the intestine which simulate so closely stricture of the duodenum. On the diagnosis depends the saving of the life. I had a case of twisting which operative procedure relieved and also a case where pressure caused very much the same symptoms and which proved to be a tubercular mass. I believe in operative measures very strongly in children, both in laparotomies in these cases and in brain operations in the cases of cerebral hemorrhage. The great question is to make a correct diagnosis and when the diagnosis has been made early operation is indicated and we shall probably gain every year in respect to making the diagnosis in these cases.

THE MEDICAL WORK OF THE JUVENILE COURT OF COOK COUNTY, CHICAGO, ILL.

BY FRANK SPOONER CHURCHILL, M.D.,
Consulting Physician to the Court,

AND

JAMES A. BRITTON, M.D.,
Attending Physician to the Court.

The Juvenile Court is an institution established to care for the dependent and neglected children of the state and to furnish the legal machinery for the separate trial and correction of youthful transgressors of the law. As an institution it is of comparatively recent origin. True, as long ago as 1736, Massachusetts passed a law intended to provide special care for dependent and neglected children. Subsequently various statutes were enacted in different colonies and states having the same general object in view. The most important of these was a law enacted by Massachusetts in 1863, providing for the separation of child and adult criminals, and similar laws passed by New York in 1877 and 1884.

The first effective step toward the solution of these problems of the child, however, was taken by Illinois in 1899, when that state enacted the first real juvenile court law. This law was entitled "An Act to Regulate the Treatment and Control of Dependent, Neglected and Delinquent Children," contained a comprehensive definition of such children and conferred on special courts original jurisdiction concerning them. The intention and spirit of the law is that the state shall act the part of a wise and solicitous parent toward its unfortunate children who become dependent or delinquent. In accordance with this law the present Juvenile Court of Cook County (Chicago) was established about ten years ago and immediately found too abundant opportunity for the exercise of its functions. Its work and sphere of usefulness have steadily increased until now it consists of a complex but effective machinery of judges, clerks, probation officers, social workers, visiting nurses, a "Court Physician" and a "Court

Nurse," all working toward the common goal of giving proper care to these unfortunate children and seeking to strike at the root of the causes and conditions producing their dependency and delinquency.

It became evident to the workers in the Juvenile Court, after a few years, that considerable numbers of the children brought before the Court were physically defective in one or more respects and that there was need of physical, fully as much as of moral, correction. For example, a child is brought before the court, having committed some petty offence. It is found on enquiry that he *used* to go to school, but was apparently deaf or could not see well, consequently did poorly in his school work, fell behind, became discouraged, was allowed by ignorant, perhaps dissipated, parents to drop out of school and to run the streets with the inevitably mischievous result. Physical examination at the Court shows the presence of large tonsils or adenoids, or markedly defective vision due to hypermetropia or astigmatism. Correction of these defects transforms the "delinquent" into a docile child, eventually willing to return to school, there to do effective work.

The county officials, in view of these facts and acting upon the suggestions of the Court workers, decided to establish systematic examinations of these children and asked one of us (F. S. C.), as Secretary of the Children's Hospital Society, to take charge of and organize this branch of the Court work. This was done in the fall of 1907, and a staff of general physicians, specialists and trained nurses was organized, and for some time supported by private contributions. Early in this year, 1909, the positions of "Court Physician" and "Court Nurse" were officially created and filled by competitive civil service examination. The position of "Court Physician" is now held by one of us (J. A. B.).

The Court now demands that each child brought before it shall first have a physical examination made by the "Court Physician," and that the recommendations of the latter shall be written out and presented, together with the child and his other papers. These recommendations are given due weight by the judge in his final disposition of the case.

The method of procedure in the physical examination is as follows: The age, sex and nationality of each child are noted;

then a physical examination is made, special attention being paid to the following points: weight, height, girth of chest, vision, hearing, tonsils, adenoids, teeth, condition of lungs, heart and skin; presence or absence of venereal disease. The object of the examination is twofold: First, to determine the immediate condition of the child in order to make the proper recommendations to the Court; and, second, to determine, by study of large numbers of children, what, if any, relation exists between their physical condition and their delinquency. It is with this aspect of the problem that this paper deals.

The total number of children studied is 2,758—1,208 "dependents," 1,550 "delinquents." A "dependent" child is one with no, or ineffectual, home care, one whose home does not "functionate" properly. A "delinquent" child is one who has transgressed the law in some way or other. The former group comprise the children from three to eleven years inclusive, the latter group from twelve to sixteen years inclusive. There are, however, a few "delinquents" among the nine to twelve year olds. The total number of boys is 2,271; of girls, 487. They are divided among the two groups as follows:—

	Dependents.	Delinquents.
Boys	862	1,409
Girls	346	141

The great preponderance of boys among the delinquents is striking.

TABLE I.—NATIONALITIES.

	1,204 Dependents.	1,550 Delinquents.
Americans (white)	31 per cent.	27 per cent.
German	15 " "	16 " "
Polish	13 " "	14 " "
Irish	12 " "	9 " "
Italian	6 " "	7 " "
Americans (colored)	5 " "	6 " "
Bohemian	4 " "	5 " "
Jews	4 " "	6 " "
	90 per cent.	88 per cent.

Nationality.—The number of nationalities represented is manifold, as we should expect when we recall that 77 per cent. of the children in the Chicago public schools are of foreign-born parentage. A table shows in percentages the relative proportion of the different peoples. Among the dependents 90 per cent. are made up of Americans, Germans, Polish, Italians, Bohemians and Jewish, in the order named, and among the delinquents 95 per cent. are made up from the same peoples plus a few from the Swedes, Norwegians and French. The remainder in both groups is made up from the English, Dutch, Lithuanians, Scotch, Danish, Hungarians, Slavs, Canadians, Indians, Austrians, Welsh, Flemish, Swiss and Greeks, less than 1 per cent. from each nationality.

The preponderance of Americans is striking, but this preponderance is only apparent. The term "American" is misleading and includes all children born in this country whose parentage could not be determined with certainty. The great majority so classed are in reality of foreign-born parentage and properly belong among other peoples. The number of pure Americans is exceedingly small. The comparatively high proportion of German (16 per cent.), Polish (14 per cent.), and Irish (12 per cent.) children is undoubtedly due to the large number of these peoples in Chicago. The school census of 1908 gives the following figures as to their numbers: Germans, 421,615; Polish, 129,733; Irish, 179,845. The actual number of children from these various nationalities brought into the Juvenile Court, and the proportion of such children to their totals in the city, may then be tabulated as follows:—

TABLE II.

Total German	Dependents,	180, or 1:2,340	of total German	population.
" "	Delinquents,	248, or 1:1,700	" "	"
Total Polish	Dependents,	156, or 1:831	" Polish	"
" "	Delinquents,	217, or 1:598	" "	"
Total Irish	Dependents,	144, or 1:1,248	" Irish	"
" "	Delinquents,	139, or 1:1,294	" "	"

Thus, the Polish people, in proportion to their total number in the city, contribute the largest number of children to the Juvenile Court work. But the number of children from each nationality brought into Court is not dependent upon mere totals alone; thus the Swedes, who come numerically next to the above three

nationalities, from a total population of 107,280, contribute not a single child to the dependent group, and furnish to the delinquent group but 31 children (2 per cent.), or 1:3,461 of the total population. Other factors than numbers enter into the problem, but what part national habits, home training, religious instruction, etc., play in the problem it is not the purpose of this paper to discuss.

Height, Weight and Chest Capacity.—The height, weight and chest capacity of individuals give a fairly correct idea of their physical vigor. Observations upon these characteristics have, therefore, been made upon the Court children, the results tabulated

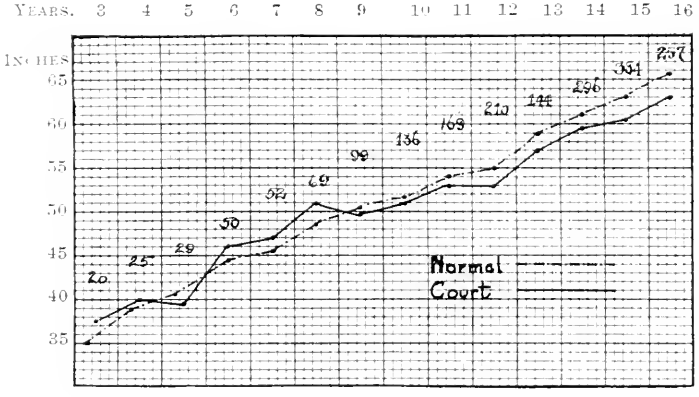


FIG. 1. CHART SHOWING HEIGHT OF BOYS.

and averaged according to age and curves plotted upon these averages. For purposes of comparison we have introduced into the charts, curves based upon similar averages obtained by Bowditch twenty years ago, studying Boston school children of American parentage.

A.—Boys.—During the first two or three years there is but little difference in physique between the Court boys and the normal, but at four or five years their poor food and unhygienic surroundings begin to make themselves felt and they fall off in weight and chest capacity. They continue slightly taller, however, until about the eighth year, when there is a sharp falling off in their rate of growth, and from this age upward they are shorter than normal. This is undoubtedly due to the fact that at about eight years many of these little fellows begin outside work, be-

sides school, getting up at four or five o'clock in the morning to buy and sell papers, and also working evenings.

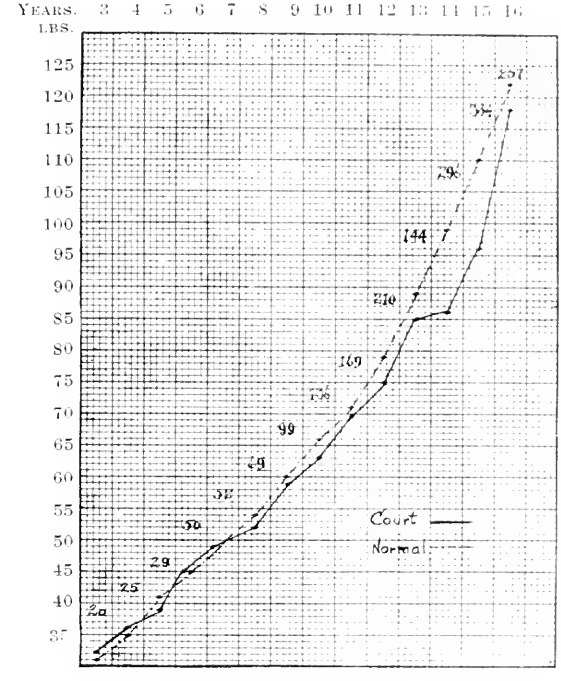


FIG. 2. CHART SHOWING WEIGHT OF BOYS

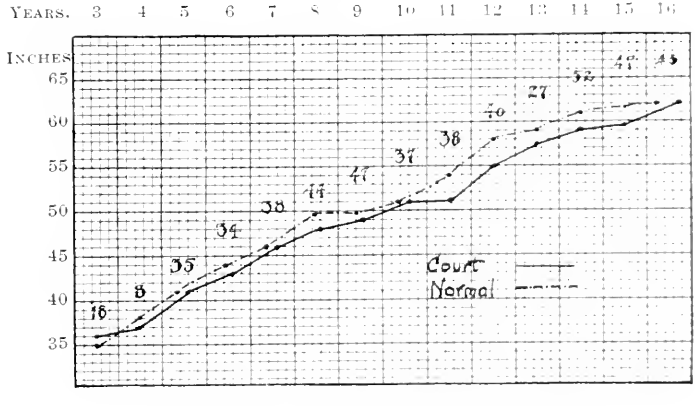


FIG. 3. CHART SHOWING HEIGHT OF GIRLS

A.—Girls.—The Court girls from the start are slightly below normal in height and weight, though apparently of greater

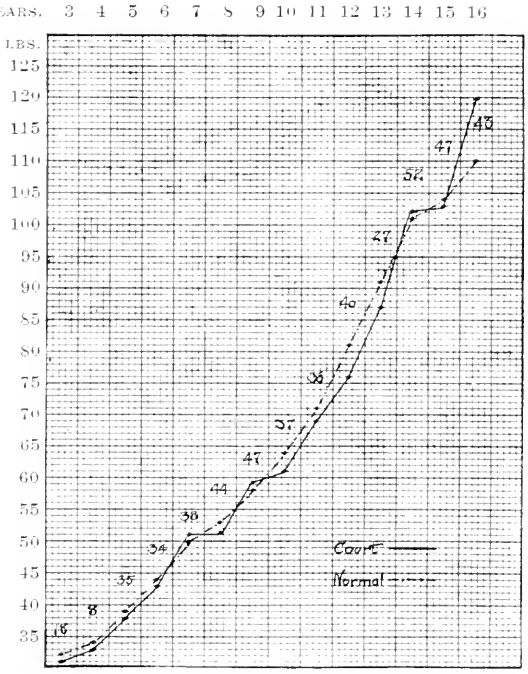


FIG. 4. CHART SHOWING WEIGHT OF GIRLS.

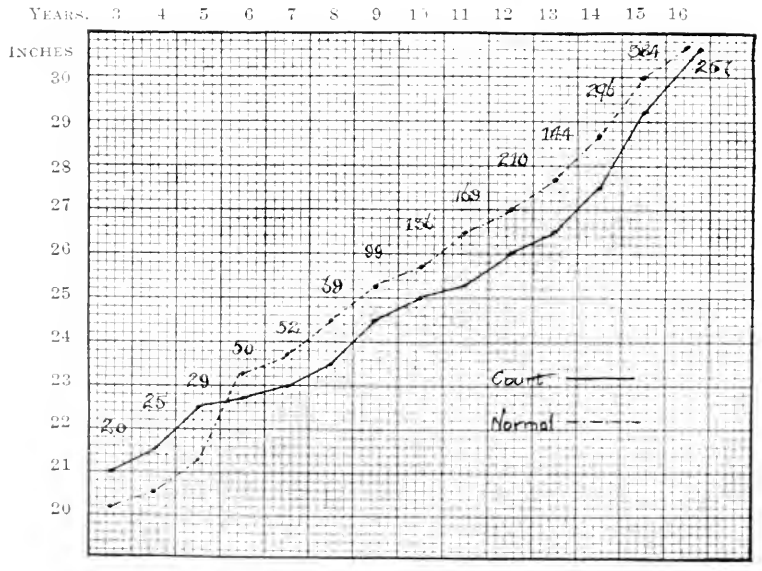


FIG. 5. CHART SHOWING CHEST CIRCUMFERENCE IN BOYS.

chest capacity at first. They continue below in all respects until the sixteenth year, when we see a sharp rise in the weight curve, and we find these girls 10 pounds heavier than normal, and of equal height. At this age they are all in the delinquent group.

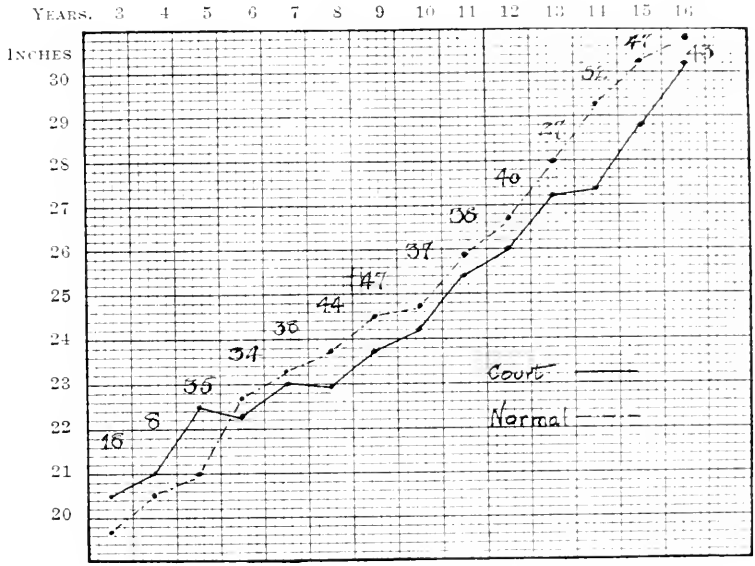


FIG. 6. CHART SHOWING CHEST CIRCUMFERENCE IN GIRLS.

are of large, well-developed form and physique, characteristics which render them attractive and lead to their delinquency.

We now come to a consideration of the abnormalities of physique in these children, studied and tabulated according to group. The table shows the proportion of the various abnormalities on the part of the different organs in both groups. The striking data are: the high percentages of carious teeth, of enlarged tonsils, especially among the delinquents, of enlarged cervical glands, of defective eyes, the large number of ear affections and the frequency of gonococcus disease. The discrepancy between the number of tonsils (45 per cent.) and adenoids (3 per cent.) is due perhaps to lack of more systematic examination for the latter. They are found in this series in only about $\frac{1}{14}$ of the cases with hypertrophied tonsils, whereas, as a general rule, adenoids are found in about $\frac{1}{3}$ of the cases having hypertrophied tonsils.

The small number of pulmonary affections (4 per cent.) is

rather striking, when we consider the surroundings of these children; only 5 cases of pulmonary tuberculosis were detected in the whole series. It is not improbable, however, that some of the children with enlarged cervical glands had tuberculosis of the lymphatic system; no tuberculin tests have been made.

There is a comparatively high percentage of ear troubles (45 per cent.); they were mostly cases of chronic otitis media, with a more or less intermittent discharge and were more frequent among the older children. There is possibly some connection between this frequency of ear troubles and the large number of hypertrophied tonsils.

TABLE III.

	1,204 Dependents.	1,550 Delinquents.
Adenoids	4 per cent.	3 per cent.
Hypertrophied tonsils	39 " "	45 " "
Cervical glands	24 " "	17 " "
Thyroid	3 " "	45 " "
Ear	8 " "	25 " "
Teeth	60 " "	68 " "
Lungs	4 " "	3 " "
Skin	4 " "	2 " "
Heart	4 " "	3 " "
Eye		17 " "
Gonococcus		35 " "

Eye defects were serious enough to require glasses in 17 per cent. of the cases, and it is not at all improbable that in the children presenting this condition of the eyes the defect may have been a considerable factor in the development of delinquency in the manner already outlined—difficulty at school, dropping out, the street, mischief, juvenile court.

The condition of the teeth, as in all dispensary patients, is most lamentable. The statement in percentages hardly gives a correct idea of the extent of this trouble, inasmuch as every child included in the record had from 2 to 12 carious teeth. The tooth-brush is a practically unknown weapon of civilization among these people, and the "tooth-brush" drill, now practiced in certain Chicago schools, should be heartily encouraged.

The Gonococcus.—Gonococcus infections are limited almost exclusively to the girls, but among them it is very frequent, 35 per cent., or more than one-third of the girls, having a gonococcus vaginitis. They were all, with but few exceptions, among the delinquents. Some had a vaginal discharge, more did not. A bacteriologic examination of the vagina is made on every girl who goes into the Court Home, where the children are kept pending their respective cases. By means of this routine bacteriologic examination not a few cases of gonococcus disease are discovered which would otherwise escape detection. The same practice is pursued with all our girl patients at the Cook County Hospital with similar results.

The absence of syphilis is somewhat surprising, only 1 case among the delinquents, and that in a fifteen-year-old boy. It is possible that cases of obscure, late, congenital syphilis among the dependents have been overlooked, but there were no self-evident cases.

The almost entire absence of acute disease, contagious and noncontagious, is due simply to the fact that we have instructed probation officers that, on no account, are they to bring a sick child to the Court Dispensary; if he be sick at the time set for his hearing the case is postponed. Rigid enforcement of this rule has undoubtedly done much to prevent the spread of disease from this particular center as a focus.

These two groups of children, taken as a whole, must be regarded as remarkably free from serious organic disease, considering their environment—air, food, lack of cleanliness. We find practically no cerebral or nervous disease, remarkably little pulmonary or cardiac disease, practically no serious digestive disturbances. The most frequent departures from the normal appear to be on the part of the teeth, tonsils, ears and eyes. All the defects in these directions are remediable, and in all instances recommendations in this direction are made, and whenever possible the children are followed up until the recommendations have been carried out. This has been possible in many cases through the coöperation of judge, probation officers, and especially of visiting nurses, the latter following the children into the homes when they are returned to the home, or into the various institutions to which they are assigned.

The frequency of gonococcus disease among the older girls is but an index of the extent to which this infection prevails among

this class of the community in every large city. It is a result, not a cause, of delinquency. That it is on the increase in our large cities there can be but little doubt, and it has already reached such proportions as to demand the attention of public officials. The Chicago Health Department has already taken measures to have cases of this contagious disease reported as the first step toward its attempted control. Physicians on the staffs of children's hospitals, of orphan asylums, etc., should coöperate in every way, especially by regular, routine bacteriologic examination of the vagina in all their female patients.

SUMMARY.

The main points in this paper may be summarized briefly as follows:—

The children brought before the Chicago Juvenile Court, in the early years of their dependency, differ little in their physique from children living under more favorable conditions. The boys, as a rule, are taller, but less well nourished and of smaller chest capacity. The girls are inferior in height, weight and chest capacity. The boys, in the latter half of their dependency, *e.g.*, nine to twelve years, and throughout their delinquency, are inferior in all respects, and their inferiority is more marked after the age of puberty. The same comment holds true for the girls during these same periods, until they reach sixteen, when they are equally as tall as, and far heavier than, their more fortunately surrounded mates, which superiority in physique, by its very attractiveness, is one of the main factors in causing their delinquency.

All the children of both groups are remarkably free from organic disease.

Considerable numbers present drawbacks to perfect physical health in the shape of enlarged tonsils, and probably adenoids, carious teeth, enlarged cervical glands and diseases of the ear. A small proportion, about $\frac{1}{6}$, have trouble with the eyes. This defective vision and the deafness resulting from enlarged tonsils, adenoid growths and ear disease, are defects more or less predisposing to delinquency by rendering school work more difficult, and the street life, with its attendant evils, more easy and attractive.

The frequency of gonococcus disease among the older girls is

merely an index of the prevalence of this infection among this particular class as a whole.

The generally lowered tone of the delinquents, as manifested by diminished height, weight and chest capacity, probably plays no part in the production of delinquency, but is rather a result of the general life led by these children, the result of common causes leading also to mischievous, delinquent conduct, viz.: bad air, poor food, bad surroundings, smoking, drinking, etc., etc.

Four thousand of these children are annually brought before the Juvenile Court of Chicago. Presumably, Chicago is no exception to the other large cities of the country. The children themselves merit whatever help can be rendered them. That their presence in such large numbers in the various centers of our country constitutes a focus of danger to the future welfare of the country, affecting seriously as it does the growing generation, there can be no doubt. They constitute a problem well worthy the serious study of all workers among children, settlement workers, nurses and pediatricians.

Our thanks are due to Miss Bena M. Henderson, Superintendent of the Children's Hospital Society, and to Miss Margaret Little, the official "Court Nurse," for much valuable help in the preparation of this paper.

Chicago, May, 1909.

THE STOOLS OF THE NEWBORN AND THEIR SIGNIFICANCE.

BY THOMAS S. SOUTHWORTH, M.D.,
New York City.

The description of the stools of the newborn infant, as it appears in the text books of obstetrics and pediatrics, consists of slight variations from a stereotyped conception which has apparently been handed down without any recent painstaking investigation. The following paragraph from a recently revised edition of an important British text book on pediatrics† may be quoted as giving the usual conception of such stools:

"The meconium which the infant passes during the first three or four days is of a dark greenish-brown color, of a viscid semi-solid consistence, slightly acid in reaction, and without odor. At birth it is sterile; but within a few hours microorganisms find their way into it through the anus. It contains epithelial cells and mucus from the intestine, and its color is due to bile, but the bulk of it is believed to be mainly composed of matter derived from the vernix caseosa which the infant has swallowed during intra-uterine life along with liquor amnii. (Zweifel.) . . . . After four, or, at most, five days the motions cease to contain meconium and assume the character of normal infantile feces. In a healthy breast-fed infant the motions are from two to four in number daily during the first month or two, and usually two or sometimes only one daily after that. They are of an orange-yellow color, and of a uniform semi-solid consistence."

From one of our most popular text-books on obstetrics‡ I take the following extract: "After the third or fourth day with the establishment of the mammary secretion, the meconium disappears, and its place is taken by feces which are light yellow in color, homogeneous in consistence, and possess a characteristic odor. For the first few days the stools are not formed, but after a short time they take on the characteristic cylindrical shape. . . . . The physician should make it a rule to inspect the stools at each visit, ... as in this way important information may be gained concerning the digestion of the child."

† "Guide to the Clinical Examination and Treatment of Sick Children." Second Edition, 1908. By John Thompson, M.D., Edinburgh.

‡ "Obstetrics." J. Whitridge Williams, M.D., 1908.

These two quotations, which are fairly representative, place before us the usual present-day teaching on this subject. Summed up in a few words this is that, after a period varying from three to at most five days, the meconium gives way to yellow or orange-yellow stools, and that these are of a uniform homogeneous, semi-solid consistency, that this is the normal stool and that any material departure from this classical type indicates a disturbance of digestion on the part of the infant or a deficient or unsuitable secretion of breast milk on the part of the mother.

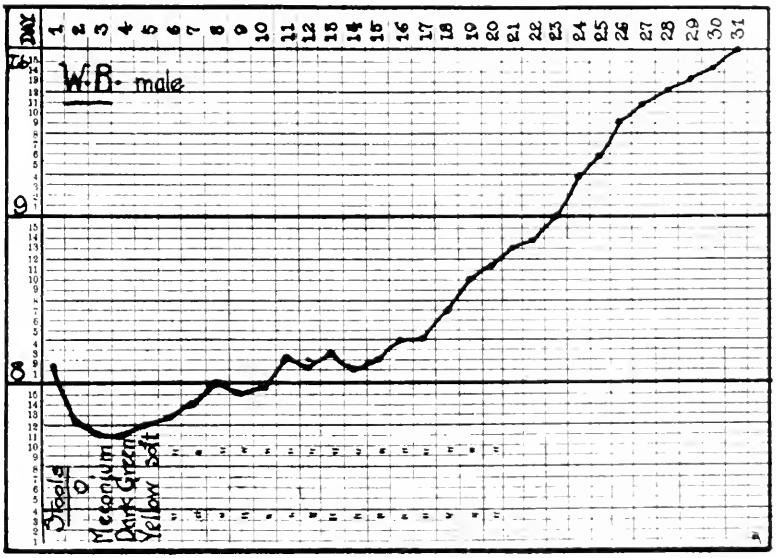


CHART No. 1—Classic stools, but slow gain. Mother had localized phlebitis. Rapid rise began on 15th day.

My observations of the stools of newborn infants in the maternity wards of the Nursery and Child's Hospital, which have extended over a number of years, are not in accordance with this accepted view, and I am inclined to believe that the reason for its persistence in the text books is due to the fact that matters pertaining to the newly-born infant lie in that debatable territory which is not occupied exclusively by either the obstetrician or the pediatrician.

In pursuit of this question, I have personally tabulated the data offered by the maternity ward charts of 50 infants born in our institution, choosing only those whose subsequent gains in weight

upon the breast alone showed that their mothers were capable of furnishing an adequate amount of breast milk. This choice was important because it eliminated the question of the color and character of the infants' stools being due to a seriously defective or insufficient secretion.

STATISTICS OF 50 INFANTS SUCCESSFULLY BREAST-FED.

		Average.
Birth weight	from 4 lbs. 2 oz. to 9 lbs. 10 oz.	7 lbs. 8 oz.
Initial loss	" 3½ " " 17 "	8¾ "
Lowest weight	on 2d to 7th day.	3 3-5 days.
Birth weight regained	" 5th " 22d "	9 3-5 "

Regained by	7th day.	14 cases.
" "	8th to 10th "	15 "
" "	11th " 15th "	16 "
" "	over 15 days.	5 "

		Average.
Rapid gain established	on 4th to 18th day.	On 8½ day.
Established by	7th "	18 cases.
"	on 8th " 10th "	17 "
"	" 11th " 14th "	8 "
"	" 15th " 18th "	7 "

COLOR AND NATURE OF STOOLS OF SAME INFANTS.

Meconium	lasting 2-5 days in 50 cases.	Average 3½ days.
	Appearing	Average cessation on
Brown	3d to 5th days " 14 "	4½ days.
Dark-green	4th " 7th " " 31 "	4th day.
Yellowish-green	5th " 11th " " 7 "	8th "
Greenish-yellow	5th " 12th " " 29 "	7th "
Greenish-yellow soft residue	5th " 18th " " 28 "	8¾ days.
Yellow soft residue	5th " 20th " " 46 "	Av. estab'ed on 9½ day.

Established by	7th day.	18 cases
"	on 8th " 10th days.	15 "
"	" 11th " 14th "	5 "
"	" 11th " 14th "	9 "
"	over 14 "	4 "

Yellow loose in 3 cases continuing until 12th, 19th, and 34th days, respectively.

NOTE.—Averages of birth weights, initial loss and day of lowest weight show the cases to be normal ones. Day birth weight is regained shows that this may be late without prejudice to successful nursing. Average day upon which a definite sustained rise in weight is begun shows that this is possible after a considerable period of adjustment has elapsed. The color and nature of the stools show the common intermediate stages between the meconium and the classic soft yellow stool, and the late appearance of the latter as opposed to the usual teaching.

The earliest stools of the infant consist of meconium and are of a brownish color. These persist for three or four days, and are followed by bile-tinged mucoid intestinal secretion, which may be dark-brown or brown-yellow, but much more commonly dark-green, depending upon whether the bile-pigment is in the form of bilirubin or biliverdin. About the sixth day a certain proportion may assume the classic yellow or orange-yellow color as the exception rather than the rule, but the majority are greenish-yellow, still more or less fluid, gaining gradually in consistency, and only at a later period—which varies considerably—becoming yellow.

The change therefore from meconium to the classic yellow, semi-solid stool is not, as the student might suppose, an abrupt one, but, as it physiologically should be, a gradual one, and the color, fluidity, and consistency, follow no fixed rule. The early scanty milk residue appears in the stools in small flakes, for which the term "curd" is misleading, because this carries with it the suggestion of imperfect digestion. In the fluid or semi-fluid stools, which are not uncommon and scarcely abnormal at this period,

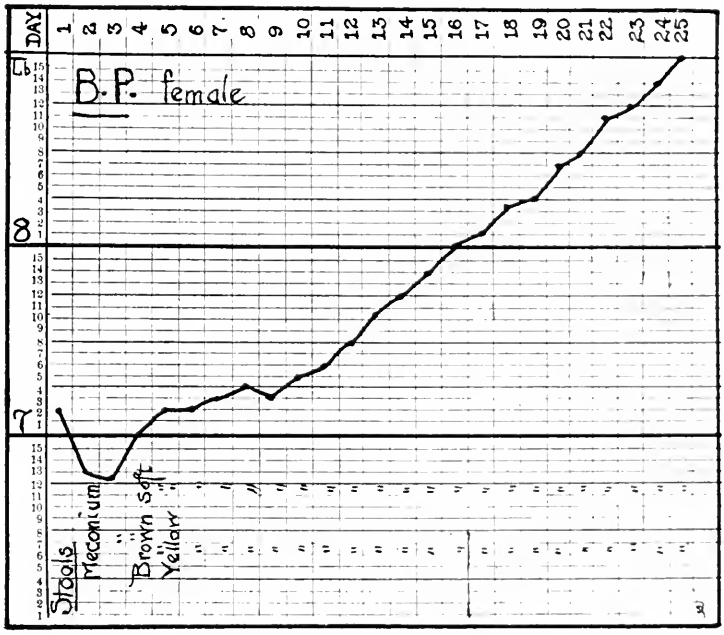


CHART No. 2—Classic stools. Infant a poor nurser. Delay in establishing physiological accord. Rapid rise began 10th day.

these flakes may hardly be recognizable, but in the semi-solid stools they are at first more or less separated by the viscid, mucoid secretion of the intestine. As the milk residue increases, the intestinal secretion, as a rule, decreases, allowing aggregation of the flakes into fairly homogeneous masses.

The importance of this variability of the stools in the newly-born lies in the prognostic and diagnostic significance which may be attached to them. Suffice it to say that under the present teaching the conclusions drawn are often most erroneous, and that in consequence steps are often taken which have a serious bearing

upon the welfare of the infant. I am personally convinced that the larger part of these variations are physiological, pending the establishment of complete harmony between mother and child in the extra-uterine phase of nutrition, and that they are by no means pathological. While they should be carefully noted and may suggest indications for minor changes in the management of mother and child, they should not be taken too seriously unless there is overwhelming confirmation in other directions.

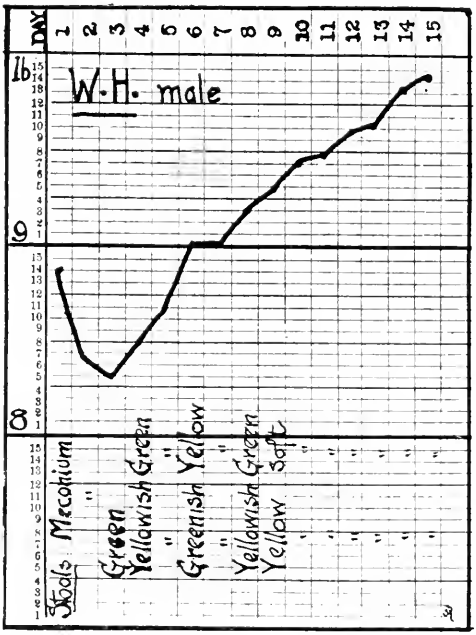


CHART NO. 3—A common type of stools. Yellow soft stools appearing on 9th day. Gain in weight prompt.

It would perhaps be natural to expect that the weight curve would furnish the most important guide at this juncture, but this also is often non-committal. Excepting in those extremely rare cases where there is no secretion from the breasts, or where sufficient nutriment cannot be obtained by the infant to maintain the body weight after the fourth or fifth day, it is only occasionally possible to make a reliable prognosis.

In vigorous infants who are able to secure promptly from the maternal breast suitable quantities of milk, and in whom after the initial drop the weight curve turns sharply upward and the birth

weight is quickly regained, the classic stool may appear more promptly, but so many factors influence the amount of the milk which the newly-born infant gets from a perfectly competent breast that it is very frequently impossible to tell either from the weight chart or from the stools, for seven or ten days or even longer, whether the infant will subsequently thrive on its mother's breast alone. Indifference, lack of vigor, or difficulty in seizing the nipple on the part of the infant; depressed, or tender and

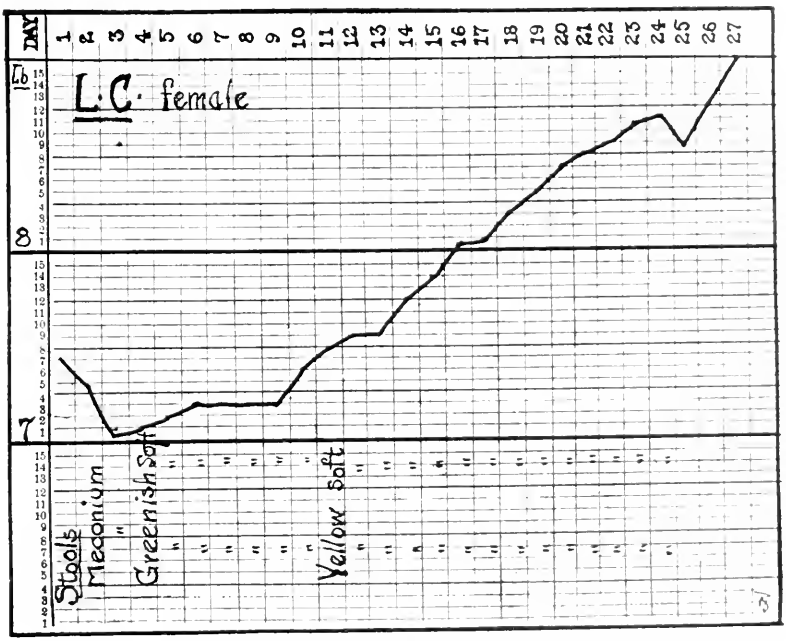


CHART No. 4.—A common type of stools. Yellow soft stools on 11th day. Right nipple retracted. Gain deferred to 10th day.

abraded nipples, ignorant methods of nursing, or temporary disturbances on the part of the mother, may all be factors which hold the infant back until the art of successful nursing is acquired by both.

Without the weight curve and with inspection of the stools alone, one is quite at sea as to the progress of the infant; but even with the weight curve and inspection of the stools, one is often still at a loss for a considerable period if the weight curve is irregular and hesitating. With such a curve there may be yellow stools and yet the infant may eventually require supplementary

feedings to start it upward. Or the stools may be of atypical color and consistency, but an improvement in all directions takes place when the mother begins to sit up and has regained some strength. With a rapidly rising weight curve the character of the stools may be practically disregarded.

We are still influenced by another former fallacy, which is, that the birth weight should be regained at the end of a week. This occasionally happens, and involves rapid and immediate gains

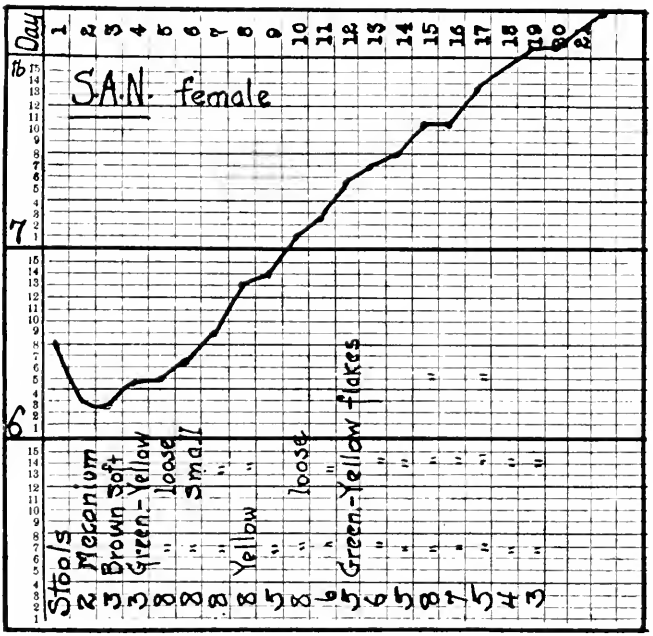


CHART NO. 5—Atypical stools. Frequent stools. Rapid gains.

succeeding the initial drop, but numerous observers have proven that this is not the rule, and that the time for regaining the birth weight is later in the majority of infants. I would particularly emphasize the fact that it may be considerably postponed without necessarily prejudicing the successful establishment of breast feeding.

The teaching—which to the best of my knowledge we owe to Dr. Holt—to wit, that frequent dark-green stools are an evidence of insufficient nourishment, has been a valuable one, but it is frequently misunderstood and misapplied. The stools thus described are a reversion to the fetal or meconium stage, and this reversion

occurs when the intake of milk is so scanty that practically no milk residue reaches the rectum and is expelled from the bowel. Such stools consist almost exclusively of the mucoid secretion from the intestine. When they continue too long after birth, or, equally important and still more striking, when at any period of lactation they follow upon stools which have previously contained milk residue, they are a valuable indication that the infant is receiving or securing a very limited or insufficient amount of food.

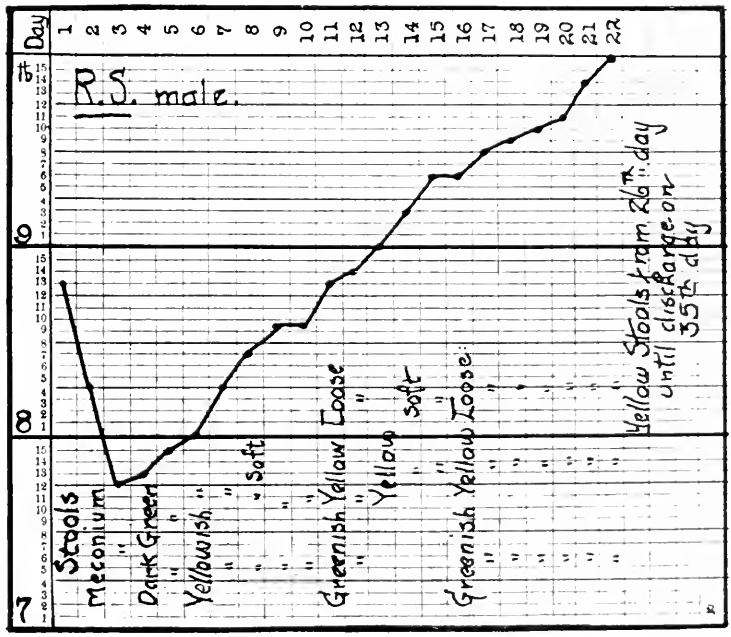


CHART No. 6—Atypical stools. Rapid gain in weight despite loose stools.

The error which has attached itself to their interpretation is the old one of assuming that any green color of the stools necessarily indicates some disturbance of the infant's digestion. Such dark green mucoid stools lose their characteristics and cease when a sufficient food residue passes through the intestinal tract. They disappear with a larger ingestion of breast milk, or, where necessary, with the addition of supplementary feedings.

These dark green mucoid stools should not be confused with lighter green stools in which the food residue, rather than the mucoid intestinal secretion, bears the green color. These may or

may not be of significance. A light greenish-yellow tinge, as I have shown above, is a very common transition stage, of longer or shorter duration, in the stools of the newly-born before they finally take on the classic yellow color. They have no relation to the green stools of summer diarrhea with which they are frequently confounded and the infant consequently subjected to utterly unnecessary and disturbing medication. Such a color is by no means inconsistent with a steady increase in the infant's weight.

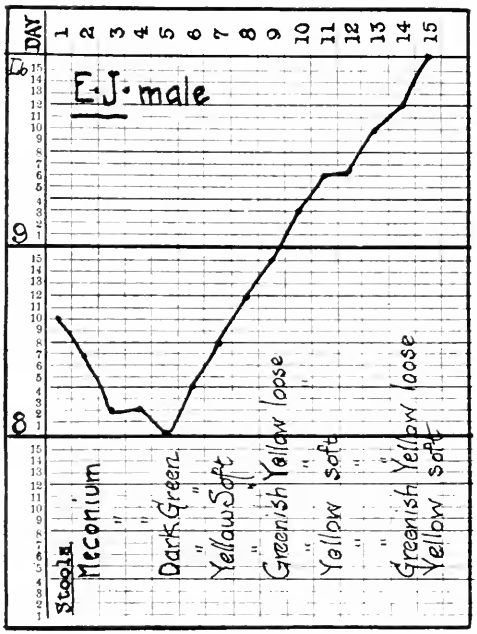


CHART No. 7—Atypical stools with phenomenal gains

An estimate of the diagnostic value of the newly-born infant's stool, from the standpoint of its digestion, should be based upon the color in the interior of the large or small masses of food residue. This is readily ascertained by smoothing them out upon the napkin. When this color is yellow the superficial color, or the color of the material by which they are surrounded, is of little importance, for it is certain that there is no serious disturbance of digestion.

One of the chief interests, however, of this investigation has lain in the discovery that so many infants continue to make satisfactory progress in weight with atypical stools. This is particu-

larly true of those infants who have loose stools which soak into the napkin, leaving little definite residue upon its surface; such remains of flaky residue as do appear in indistinct form more often having a yellow color, but not infrequently in places a greenish-yellow, light-green, or even white color, without seeming influence upon the progress in weight. A certain number of these infants I have found to be receiving a breast milk containing a high percentage of fat, but I am not yet fully prepared to say that that is

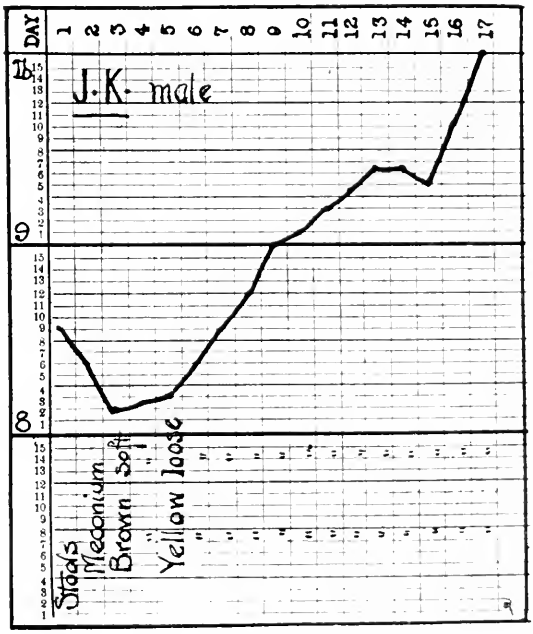


CHART NO. 8.—Loose yellow stools. Excellent gain-

always the case. Common sense measures, which are too often ignored or neglected, will often serve to bring about the physiological adjustment between mother and infant, where at first this may seem unpromising.

CONCLUSIONS.

The classical orange-yellow, semi-solid stools appear in the majority of newly born infants later than is usually stated.

Considerable variations in the color and consistency of the stools of newly born infants are entirely compatible with regular gains in weight.

Such variations in the stools for ten days or more, even with slow, halting, or irregular gains in weight, give no certain indication that successful breast feeding cannot be carried on.

Whatever the character of the stools, weaning is never indicated if the infant is gaining steadily in weight; nor with delayed gains until intelligent efforts have been made to bring mother and infant into physiologic accord.

In the dark-green mucoid stools of insufficient nutrition, which are starvation stools, and not limited in their occurrence to the first few days of life, there is a practical absence of milk residue.

A good yellow color of the masses of milk residue, or a yellow color when they are smoothed out, precludes the assumption of indigestion, whatever the color of the exterior or of the surrounding medium.

Delayed gains in weight are very often the result of factors which prevent the infant from securing a sufficient quantity of milk from perfectly competent breasts, and with patience and ingenuity these difficulties may be overcome.

807 Madison Avenue.

DISCUSSION.

DR. MORSE.—I am inclined to think that we shall not fully understand these apparent abnormalities in the stools in the first few weeks of life until we combine our observations of the stools with simultaneous chemical examinations of the breast milk. I think that we shall find that the variations in the stools depend upon the composition of the breast milk. The early brown color corresponds to the colostrum stage. Later the color depends on whether bilirubin or hydrobilirubin is present. The color also depends upon the relation between the amount of fat and proteids in the milk.

DR. GRIFFITH.—The first chart interested me because it bears out the conclusion that Dr. Gittings and I communicated to the Society a couple of years ago, that there was nothing to be gained by early forcing of the infant's feeding: for instance, by the breast of another woman. It looks as though the child required a little time to adapt itself. At the end of two weeks the gain is no greater in those fed from the day of birth and those who went hungry until the mother's secretion was established.

I thoroughly agree that the child's general condition of health and gain is of more importance than the character of the stools. Change in the stools may give a warning, but it is not a positive indication to stop the feeding.

DR. WINTERS.—This paper of Dr. Southworth's should be placed in the hands of every obstetrical nurse in America. They are totally ignorant of these indications, and because the stools are a little green they think something is very wrong, and, of course, they have great influence with the mother.

DR. SHAW.—Twelve years ago Epstein told his clinic that it was almost impossible to show the so-called classical normal stool. We see many babies that are doing perfectly well with apparently slightly abnormal stools.

DR. FREEMAN.—I am in accord with what Dr. Southworth has said about these cases. The results, however, in private practice, are much better than his showing with the hospital infants. A great proportion regain their weight at the end of a week. If the baby is gaining in weight you can ignore some irregularities in the character of the stools.

DR. SOUTHWORTH (closing remarks).—I think we all recognize these conditions, but I have not been able to find that the subject has been crystallized in this form. I tried to pick out a chart in which the gain was immediate and sharp after the initial drop and accompanied by the classical stool on the third or fourth day, and could not find one in over one hundred hospital charts. The chief difficulty is, as Dr. Winters has said, that the color and nature of these stools has led to misapprehension and a good deal of unnecessary advice concerning weaning.

A SPECIMEN OF FORMED MECONIUM SIMULATING INTESTINE.

BY ROWLAND G. FREEMAN, M.D.,
New York.

A year ago I was called in great haste to see a case in consultation with a physician who told me that he had delivered a woman of a healthy baby that morning; that the baby's bowel had since protruded from the anus and that it was turning black. On inquiry he replied that otherwise the child was apparently in perfectly good condition.

On reaching the case I found a robust looking baby, and as the doctor had said from the anus there protruded a cylindrical coil of dark material with a shiny surface about one-half an inch thick and some eight inches long, having the dark greenish-brown color characteristic of meconium, one free end telling the story that it was not intestine.

Figure showing how formed meconium gave the appearance of gangrenous intestine.

On very gentle manipulation close to the anus, this mass was separated, which proved to be simply formed meconium consisting of the ordinary material of meconium, desquamated epithelium, and bile with a dried mucous covering.

A specimen of this somewhat distorted by having soaked in fluids for a long period and a photograph of a portion of it will give some slight idea of its appearance.

THE BACTERIOLOGY OF ACUTE INFECTIONS OF THE RESPIRATORY TRACT IN CHILDREN, WITH ESPECIAL REFERENCE TO INFLUENZA.

BY L. EMMETT HOLT, M.D., LL.D.,

Professor of Diseases of Children, College of Physicians and Surgeons (Columbia University), New York.

One of the most striking changes in the mortality figures of the last twenty-five years is the increase in the deaths from acute respiratory diseases, especially bronchitis and pneumonia. This has affected all ages, infants and young children as well as adults. As respects infants, the mortality from respiratory diseases in New York City touched the high point in 1902, since which time it has fallen but little. This increase is in striking contrast with the steady decline in acute infections of the gastrointestinal tract, these having fallen almost as steadily as those of the respiratory tract have risen. Thus twenty-five years ago the deaths under one year from acute intestinal diseases numbered 3,300, the same years those of bronchitis and pneumonia only 1,350. In 1902 the deaths from acute respiratory diseases were greater by nearly one hundred than those from acute intestinal diseases the following year. Nowhere has this increase been more marked than in institutions, especially in those for infants and young children. Nothing in a hospital service is more discouraging than the development of such complications as acute nasopharyngitis and otitis, laryngitis, bronchitis and bronchopneumonia in children admitted for other diseases or for surgical operations. In fact these infections have come to be almost as serious as the development of scarlet fever, measles or diphtheria, and often more difficult to control, because the source of infection and manner of spreading are less obvious.

In common with others I had shared the opinion that for the increase in acute respiratory diseases, influenza especially was largely responsible. In order to determine what part this actually played in our respiratory infections systematic observations were begun in the Babies' Hospital in December, 1908, and continued through the following winter and spring with some observations during the summer months for control.

In working out this problem I have been fortunate in having

the assistance of our pathologist, Dr. Martha Wollstein, who for several years has devoted much time to the study of the bacillus of influenza and allied organisms. This fact is a sufficient guarantee of the accuracy of the bacteriologic part of this report. I desire also to express my obligations to Dr. Josephine Hemenway for valuable assistance in carrying out this investigation.

Realizing the vagueness with which the term influenza is used, we endeavored to discover how much real influenza existed which could be demonstrated by cultures, accepting nothing else as of diagnostic value except the presence of Pfeiffer's bacillus.

We began by making cultures from all suspicious cases, *i.e.*, patients with moderate or severe catarrhal inflammation of the upper and lower respiratory tract, and cases with high temperature without obvious explanation. Later this was extended to cover all respiratory catarrhs and all cases of pneumonia, empyema, otitis and all febrile cases of doubtful diagnosis, particularly those developing in the wards. At times cultures were taken from all the patients of certain wards, also from caretakers, nurses and physicians who were in contact with the children. Finally, cultures were made from the lungs and heart's blood at all autopsies whether influenza was suspected or not. It is scarcely necessary to state that the bacillus of influenza grows only upon a medium containing hemoglobin, that of the rabbit's blood being found to be best. Blood agar plates were used for all our cultures.

It developed in the course of these observations that success depended in no small degree upon the manner of taking the cultures. It was found that the bacilli were often difficult to obtain unless bronchial secretion could be secured. The cultures made therefore are to be regarded as sputum cultures rather than simple throat cultures. It was not always easy to secure sputum from the infants who composed most of our patients, but by following the plan pursued at the hospital for obtaining tuberculous sputum, results were much improved. The child was made to cough by irritating its pharynx with a bit of gauze or cotton in the jaws of an artery clamp, and any secretion brought into view was secured upon this swab. Not all children could be made to cough well and hence the explanation of some negative results in positive cases.

I have to report upon observations made upon 36 nurses, 4 physicians, and about 250 children, repeated cultures having been taken from many of the persons. In addition to these obser-

vations upon patients, cultures were made from the lungs and heart's blood in 59 consecutive autopsies.

In the records of throat cultures no account has been taken of the micrococcus catarrhalis since this was found to be present in practically every child examined whether symptoms existed or not, and at first no account also of the staphylococcus. The streptococcus was noted when present and also the pneumococcus. In the first few cases the latter was noted only when present in considerable numbers, but in the latter part of the season—including nearly three-fourths of the cases—the presence of the pneumococcus was noted in every instance where it was observed.

The B. influenzæ was discovered by cultures in 85 persons, of whom 42 were suspected of having influenza and 43 were not, 15 of the latter being nurses or physicians who were in intimate contact with patients.

A suspicion of influenza in the 42 cases rested upon the following symptoms:

Obscure high temperature with few or no catarrhal symptoms	12
High and widely fluctuating temperature with or without pneumonia	7
Acute catarrhal symptoms with fever	3
Acute catarrhal symptoms with little or no fever	8
Severe catarrhal symptoms protracted or recurring	9
Protracted bronchopneumonia	3

In the 43 non-suspected cases showing the B. influenzæ the clinical conditions was as follows:

No symptoms (routine cultures) of which 15 were nurses	10
Moderate bronchitis, young infants exposed to influenza	7
Typical pneumonia with high temperature (one had crisis)	3
Empyema (one staphylococcus and one pneumococcus)	2
Nurses with slight cough and pharyngitis	2
Various others	10

In the beginning we made many mistakes expecting to find B. influenzæ in cases of acute nasopharyngeal catarrh, but it was not found in a single one of 5 such cases developing in the wards, nor was the pneumococcus found. Our findings thus confirmed the

results of Lord in Boston, that what is popularly known as "acute influenza" is seldom due to B. influenzæ.

Of 47 cases of acute bronchitis or pneumonia occurring in the winter or spring months, the B. influenzæ was found by cultures in 63 per cent.; some of these were admitted with the disease and others developed it in the wards. I shall refer again to the clinical features of the influenza infections.

At this season, of 113 persons giving negative influenza cultures, 16 were suspected and 97 were not; 43 of the latter being routine cultures taken from persons in no way ill. It is of some interest that in 2 of the suspected cases giving negative cultures during life, the autopsy showed the B. influenzæ in the lungs.

It is, I think, very significant that while positive influenza cultures were frequently obtained through April and the early part of May, with the advent of warm weather this organism disappeared entirely from our patients. Cultures were made from 27 persons in the latter part of May and through the month of June, and but once (this in the person of a nurse who had it many times before) was the B. influenzæ found after May 20th, though in some of these patients or nurses it had been regularly found during the previous weeks.

The association of the pneumococcus with both positive and negative influenza cultures is interesting. In the winter and spring observations of the patients giving positive influenza cultures, the pneumococcus was present in 48 per cent. Of the persons giving negative influenza cultures the pneumococcus was present in 27 per cent. Of the 74 cases of acute bronchitis and pneumonia seen at this season, 46 per cent. showed the pneumococcus and 54 per cent. did not; of those same 74 cases it will be recalled that 63 per cent. had influenza and 37 did not. Although the B. influenzæ disappeared with the advent of warm weather pneumococcus infections persisted. Of 27 June cultures it was present in 10; 5 of these being cases of pneumonia and 5 had no bronchial or pulmonary symptoms, though 1 developed pneumonia a few days later. Not only were pneumococcus infections seen in the respiratory tract, but several cases of pneumococcus ophthalmia occurred in the hospital during the summer. Our influenza, then, was limited to winter and spring, but pneumococcus infections persisted throughout the year.

Influenza and otitis are so often associated in the mind of the clinician that the bacteriologic findings possess some interest.

During the winter and spring season, cultures were made twenty-nine times after fresh punctures in cases of acute otitis. The staphylococcus was found twenty-five times; the pneumococcus, nine times; streptococcus, once, and the B. influenzæ in not a single instance. Yet in throat cultures made from the same patients, 15 of the 29 showed the B. influenzæ and 3 showed the pneumococcus. This result is to be taken in connection with the findings in the cases of acute nasopharyngitis, in which also no B. influenzæ were found. These suggest that the relation of influenza to these complications resembles that of measles in that it furnishes conditions in which the microorganisms commonly present in these cavities become active and excite inflammation. In not 1 of 5 cases of acute retropharyngeal abscess was the B. influenzæ found either in the throat or the pus; these cases showed staphylococcus or streptococcus.

Postmortem Observations.—Cultures were made from the lungs' and heart's blood in 59 consecutive autopsies. Forty-seven were made in the winter and spring and 12 in the early summer. It is important to consider the two groups separately. In the winter and spring cases the B. influenzæ was found in 17; 12 of these gave positive influenza cultures during life, 2 gave negative cultures, though influenza was suspected from the clinical symptoms, and in 3 no cultures were taken. Of the 17 cases in which influenza was found, 11 were cases of acute bronchitis or acute bronchopneumonia; 2 were terminal pneumonia following marasmus; 3 were pulmonary tuberculosis, and 1 only was non-pulmonary. As in cultures made during life the pneumococcus was found less frequently, *i.e.*, in only 13 of the 47 cases; 6 of these were acute bronchitis or bronchopneumonia, 1 a terminal pneumonia with marasmus, 2 pulmonary tuberculosis, and 4 non-pulmonary cases. Considering only the 16 cases of acute bronchitis and bronchopneumonia which came to autopsy the B. influenzæ was present in 11 and the pneumococcus in only 6. In 2 cases of acute bronchopneumonia there was no growth from the lungs; in 3 cases neither the B. influenzæ nor pneumococcus was present, but only the staphylococcus. Of special interest is 1 case in which a pure growth of the B. influenzæ was obtained from the lungs, and another case in which a pure growth of this organism was obtained from a serofibrinous pleural exudate. Of the 12 autopsies made in early summer, the B. influenzæ was found in none; the pneumococcus in 6, 4 of those being cases of pneu-

monia. In 1 case of pneumonia no growth was obtained. In 4 staphylococci were present. The part played by the streptococcus in this series of pneumonia cases was not great. It was found in 4 cases only, never as the sole organism.

The Value of Cultures for Diagnosis.—The question naturally arises, How much value can be attached to cultures for the diagnosis of influenza? Some idea may be gained from our experience. Of 58 persons suspected to be suffering from influenza, the throat cultures were positive in 42 and negative in 16; but in 2 of these negative cases the organism was obtained from the lungs at autopsy. On the other hand, of 185 persons—patients or inmates of the hospitals—not suspected, the cultures were negative in 142 and positive in 43, 15 of the latter being nurses. There were only 11 persons from whom the first cultures were negative and later ones positive. Three of these gave two negative cultures before a positive one was obtained. The significance of these facts is lessened by knowing that the cultures were separated by some time, usually one to two weeks, so that it is perhaps quite as likely that a later infection occurred as that the organism had been previously present but missed.

The proportion of positive throat cultures in suspected cases (73 per cent.) is, I think, sufficiently large to warrant one in attaching considerable importance to these results. Early in our observations we classed as "suspected" cases of acute nasopharyngitis, which later experience proved to be an error. Excluding these would raise our proportion of positive results to 80 per cent. While one cannot regard a single negative culture as having the same significance as a negative culture from a throat in suspected diphtheria, two or three negative cultures, if properly taken, can, in most cases, be depended upon. Negative results in influenza are more reliable than are negative examinations for tubercle bacilli from sputum, since it is easier to demonstrate the B. influenzæ by cultures than it is to discover the tubercle bacilli by staining smears.

The Significance of the Bacteriologic Findings.—We have deferred until now a consideration of the important question which might very properly have been taken up at the outset, viz., What significance has the presence of the B. influenzæ in the pharyngeal or bronchial secretions of persons suffering from acute febrile or catarrhal symptoms, or in apparently healthy persons in whom no such symptoms exist? When present with symptoms, is it fair

to assume that it bears a causal relation to them? Is the B. influenzæ in these cases the specific pathogenic organism, or only one of an associated group which together cause the familiar symptom-complex?

That the B. influenzæ is pathogenic is, I think, now generally admitted; and I may say that, so far as these observations go, they show quite as much for the B. influenzæ as they do for the pneumococcus. It is granted that in most of these cases we are dealing with mixed infections; but I would call especial attention to 2 cases which came to autopsy, one of which showed a pure culture of the B. influenzæ in a serofibrinous pleural exudate, and in the other a pure growth of the same organism was obtained from a pneumonic lung. No blood cultures were made during life. In no case in this series of 59 autopsies was the B. influenzæ obtained from cultures made at autopsy from the heart's blood.* No case of meningitis was seen in which it was the sole organism, though a number of such cases have been reported to have occurred in Mount Sinai Hospital in New York.

My own belief is that infection with the B. influenzæ plays at the present time a very important part in acute inflammations of the respiratory tract, especially bronchitis and pneumonia. According to these observations, it apparently plays at certain seasons a more important part than the pneumococcus. The cultures indicate that the B. influenzæ and the pneumococcus are associated in a very large number of patients. I believe that this mixed infection of influenza with the pneumococcus has greatly modified the clinical course of our acute pneumonias as we see them to-day. The clinical picture may be determined in one case by a predominance of influenza; in another by a predominance of pneumococcus infection. It may be difficult and, perhaps, in most cases, impossible to separate the two factors. But that there is such a pathogenic and clinical entity as influenza, from our experience seems certain.

With respect to apparently healthy persons whose bronchial or pharyngeal secretions contain the B. influenzæ, we are in exactly the same position as with persons who in a similar way harbor the pneumococcus. Who shall say that either is not a menace to those with whom such a person is thrown in close contact? Among persons examined in the course of these observations,

* This has, however, been found by us in 1 case since that time.

there were found 16 healthy persons whose secretions showed the B. influenzæ often in pure cultures and repeatedly; 15 of these were nurses. Of these 16 persons, 8 showed also the pneumococcus and 4 had diphtheria bacilli. One nurse gave positive influenza cultures on April 30th, May 5th, 10th, 12th, 18th and 28th, but later this organism was not found. In several of these examinations there was a pure growth.

I confess I am strongly inclined to regard such persons as probable carriers of infection, to be treated accordingly. It is only by the recognition and removal of such obscure etiologic factors that we shall succeed in limiting house infections. This raises many practical questions regarding quarantine and isolation, which with our present knowledge are difficult to answer. But our eyes should certainly be open to cases of this kind, which I believe to be a very real source of danger.

Positive pneumococcus cultures without influenza were obtained in 20 apparently healthy persons, 11 being nurses; in many of them repeatedly. In about half of the healthy inmates of the hospital examined in the winter months either the pneumococcus or the B. influenzæ was found, and in one-half neither was present.

The Clinical Features of Influenza Infections.—These observations indicate that there is no single clinical type which we may always diagnosticate influenza; that it is not often the acute catarrhal symptoms of the upper respiratory tract, but certain constitutional and certain bronchopneumonic symptoms. The constitutional symptoms are often characterized by rather high and widely fluctuating temperature, but without a corresponding amount of general prostration. The temperature seems to have no regular course; it may last only two or three days or may run as many weeks, even without the development of inflammatory complications, which, however, are very prone to occur.

The bronchopneumonic symptoms are sometimes early and, apparently, primary, but more often secondary and late. They are frequently severe and threatening, but of a very evanescent character. At other times they are protracted and tend to chronicity, with frequent exacerbation of acute symptoms indicating a persistence of the infection. This latter form is frequently seen in children of feeble resistance.

There are other inflammations accompanying influenza which are to be regarded in the light of complications. A frequent one is otitis. In 85 persons giving positive influenza cultures there

were 17 of acute otitis. It will be recalled that in no one of the 29 cultures made from fresh paracentesis was the B. influenzæ found. It is easy to see, then, why this operation, while relieving the local inflammation, not only does not cause the temperature to fall to normal, but may not affect the temperature at all. The operation has no effect upon the general influenza infection. This is a point which those who regard a persistent temperature after paracentesis has been done as a sign of mastoid disease, will do well to consider. I have with difficulty on several occasions restrained the aural surgeon from opening the mastoid or excising the sinus or jugular vein—in no instance, I am happy to say, to the detriment of the patient.

I have recently seen 5 cases of acute nephritis, and rarely acute pyelitis, as a complication of influenza, especially when persistent, occurring just as they might do after scarlet fever or measles.

The B. influenzæ being chiefly found in the lower respiratory tract, it is easy to see why gargles and mouth washes proved so unsatisfactory as a means of getting rid of the organism from the secretions. When once firmly established in the bronchial mucous membrane it seems most difficult to get rid of it, especially in persons whose general condition is below par. This explains the resistant character of influenza attacks in such patients, also the recurrence when the disease is apparently at an end, and the marked influence of change of climate in curing these patients. In cases seen in the winter or spring in New York sometimes nothing but removal to a warm climate will break up these attacks, but it is surprising how quickly persons often get well when such a change is made. I once regarded these recurrences as reinfections from without, sometimes from the apartments or from other persons, but in the light of the culture findings my present belief is that such patients have never gotten rid of the organism from the bronchi, and that these under favorable conditions continue to excite relapses.

These observations shed light upon certain irregular features of acute bronchopneumonia, especially cases in which a high and widely fluctuating temperature occurs. Persistent temperature occurs from influenza infection without important signs in the chest or other evidences of local inflammation. A knowledge of these peculiarities of the temperature curve in influenza should make one very cautious in inferring that because a child with acute otitis, for instance, has a persistent and widely fluctuating

temperature, such temperature, provided pneumonia can be excluded, depends upon the ear condition or its complications, mastoiditis or sinus thrombosis.

In the management of the protracted cases nothing can be worse than crowding and the confinement in close rooms or wards. Fresh air in abundance is indispensable. Regarding the effect of very cold air in hospital wards for such patients, I am not quite so sure. If it can be secured without exciting fresh catarrhal attacks it may be desirable, but this risk is considerable. The extreme susceptibility of these patients should be remembered, and I have in several instances seen an attack of pneumonia which seemed to be traceable to such exposure to cold-air treatment. Cold wards are, I believe, of much greater value in preventing these respiratory infections from gaining a foothold than in curing them when once established.

In conclusion:—

(1) The use of the term influenza should be limited to an inflammation or infection excited by Pfeiffer's bacillus.

(2) It is of great importance in all infections of the lower respiratory tract, especially during the cold weather.

(3) It has very much less to do with acute nasopharyngeal catarrhs and their complications, otitis, adenitis, etc.

(4) It may cause severe general symptoms, with few or no local symptoms, even of the respiratory tract; but general blood infection with this organism is very rare.

(5) While in most patients the influenza infection causes acute symptoms only, recurrences are exceedingly common, and in some patients the attacks are prolonged, persisting for months.

(6) The B. influenzæ may be harbored in the secretion of apparently healthy persons for a considerable period, certainly for several weeks; whether such persons may spread the disease is not yet determined.

(7) If properly made, cultures are of great value for diagnosis, but the bronchial secretion should be secured.

The foregoing is to be regarded as a preliminary report, as the study is still going on and many more observations will be necessary before definite conclusions can be reached.

14 West Fifty-fifth Street, New York.

DISCUSSION.

DR. ROTCH.—I think this paper will lead us all to make further investigations in this disease, and it is a disease about which there is much to be learned. What Dr. Holt has given us to start with is very helpful, and we very much need to be instructed in this matter. We are apt to make the diagnosis before we should perhaps, and it is a difficult disease to deal with and needs to be dealt with in exactly this way—with careful cultural examinations. There has been some recent work which has come out of the Wright Laboratory, in the Massachusetts General Hospital, which seems to show that the organism of influenza is not the principal cause any more than are a number of other organisms. In cases, with apparently no symptoms of the disease, the bacilli were shown in large numbers, and there were only 15 per cent. of cases where the symptoms were those of epidemic influenza. They came to the conclusion that this bacillus must not be thought to be the one cause. They found that there was no one group of organisms that could be said to cause influenza and that it probably had no more to do with the disease than certain other organisms. Whether this will be proven and upheld later I do not know, but the investigators who have done this work are reliable.

DR. HOWLAND.—It seems unfortunate that the term influenza bacillus is used synonymously for Pfeiffer's bacillus, for while Pfeiffer's bacillus is apparently the cause of some epidemics which are clinically typical influenza, there have been other epidemics in which the organism was found either rarely or not at all. This has been pointed out by many excellent bacteriologists, and in this country David Davis, of Chicago, and F. T. Lord, of Boston, have especially emphasized it. It is probable that the clinical disease known as influenza can be caused by many different organisms or else the true cause has yet to be discovered. It also seems that there is a closely allied group of organisms resembling Pfeiffer's bacillus Gram-negative, and growing best upon hemoglobin containing media. These organisms are found in many different conditions in conjunction with other organisms in various inflammations of the eye, in diphtheria, pneumonia and pertussis, and while they may be harmless at times, there is no doubt that they occasionally are markedly pathogenic, as is shown by their presence alone in fatal cases of meningitis.

DR. KOPLIK.—Some time ago I took regular routine cultures in every child that came into my service for diphtheria bacilli, and of 300 children who presented normal throats, 35 had diphtheria bacilli. I stopped taking routine cultures.

In this case of the nurse there were no symptoms of influenza, and still the bacilli were found in her throat. This raises the question of the ubiquity of these organisms. What is their significance? Some of these cases had otitis, and I have recently had

a case in my ward under the care of the otitic surgeon that ran a temperature for three weeks; the child is now getting well. Pus anywhere in the body will cause temperature. Possibly the influenza bacillus would have been found in the throat of this child, but would not the otitis have caused the temperature?

DR. ROTCH.—We have proven in our wards that such persons can convey the disease, though not having it themselves.

DR. KOPLIK.—No one has yet proven that any one in perfect health, though carrying the germs of diphtheria or influenza in the throat, has conveyed the disease to others. There is an exception in the case of typhoid carriers where the intestinal material has been directly placed—contact infection. The simple carrier is not enough.

DR. ROTCH.—We think we have proven it in Boston.

DR. ABT.—Every now and again an epidemic of diphtheria occurs when the origin cannot be traced. In one epidemic we became desperate to find its source. We took cultures from every one's throat, and, finally, found that one little nurse had a great number of diphtheria bacilli in the throat. We kept her under observation for some time, placing her in the isolation department. Sometime after all this was forgotten she was taken back into the children's ward and we had the same experience—another epidemic—and she was sent away from the school for a time. I think it is the only safe course. If bacilli in the throat of a child or attendant are found we should fortify ourselves against the dangers of infection, isolating the infected individual. In our hospital every child, before being admitted, has his throat examined for diphtheria bacilli, and if they are found the child is kept in the isolation department as long as they persist.

DR. HAMILL.—Dr. Holt has certainly demonstrated a very suggestive relationship between the influenza bacillus and the prolonged and irregular temperature curves which he has shown to us. I have been observing temperature charts of this character in one of my services in Philadelphia for the past two years. I have been having cultures made from the nasal secretions and the throat, and occasionally from the blood; but I had about reached the conclusion that these recurring temperatures were dependent upon a number of different infecting organisms. The influenza bacillus was not found frequently. In one series of cases, observed during the past winter, with some difficulty, we isolated the Klebs-Loeffler bacillus. Some of these cases had markedly remitting and occasionally intermitting temperatures; whilst others would continue elevated for a period of several days and then remit.

Dr. Holt's demonstration is certainly very interesting and stimulating, and in the future I shall endeavor to give the influenza bacillus more careful consideration in connection with these cases.

DR. SOUTHWORTH.—However much diversity of opinion may exist as to the exact organism responsible, there is certainly no question that there exists a clinical entity which by exclusion we are in the habit of calling influenza, and that this condition is the source of epidemics in our hospitals. During the months of January, February and March we had none of the usual infectious diseases of childhood in the Nursery and Child's Hospital, but we had a series of epidemics of what we called influenza. The temperatures which occurred during this time were apparently chiefly due to this cause. They were accompanied by a large number of cases of ear disease, which was also confirmatory of the diagnosis. In the previous year, out of 125 children, there were 40 instances of paracentesis in three months, and last year, I am sure, an equal number of incisions were made and many of the temperatures were striking in the way in which they fell as soon as paracentesis was done. It leads me to believe that a good many of the rises in temperature during these cases are due to otitic involvement. Certainly the way in which the temperature would often go down after incision of a reddened and scarcely bulging drum was at times very striking. There was also no doubt about the infectiousness of the cases. We have all, I think, observed this in private practice, and the corollary is that influenza should be considered as much of an infectious disease as the ordinary exanthemata. The importance of prompt isolation of cases, whether occurring in adults who may infect children, or children who may infect other children, has not yet been fully recognized and should be more fully considered in the future if we are to save the children from the serious complications of this disease.

DR. MILLER.—I can sympathize with Dr. Koplik's position. Any one who has suffered from the annoyance of routine examinations of the throat must feel very much as he does. At the same time I think these people who carry diphtheria bacilli, at least, are a menace to others. We had, quite a number of years ago, in the Child's Hospital in Philadelphia an epidemic of diphtheria, and one of the residents had the bacilli in his throat for several months; they were found on examination over and over again. At last he developed clinical diphtheria. Now if he was a menace to himself, he certainly must have been a menace to others. I would like to ask if in some of these cases of Dr. Holt's the only symptom was that of fever? We have all seen cases of unaccountable temperature in infants; the febrile movement lasting for a week or more, but without other symptoms, and with no Widal reaction or signs of pneumonia. Could such cases be due to the influenza bacillus?

DR. LA FÉTRA.—One of the important things to be emphasized in the presentation of this subject is in regard to the ear involvement. Three times during the last two years I have had to stay the hand of aural surgeons from performing the mastoid and

sinus operation, because they do not seem to know that these fluctuations of temperature will often occur after paracentesis has been done. In one instance I had quite a controversy with the aural surgeon because he was determined to do an operation on the mastoid. He was quite sure that these changes of temperature must be due to pus, although he could not demonstrate it by any focal signs. The blood count, moreover, would tend to confirm the surgeon in his determination to perform the operation because these patients are apt to have a high polymorphonuclear count. Unless, however, the aural surgeon can be confident from the local signs that the mastoid is involved, he should in my opinion be allowed to operate. General symptoms, and particularly the temperature fluctuations, are not a safe guide in these influenza cases.

DR. SOUTHWORTH.—I did not intend to imply that the temperature always came down after a paracentesis, and I fully agree with Dr. La Fétra's position. When the aural surgeons better understand the course of influenza in children there will be fewer operations upon the mastoid.

DR. SHAW.—As to the technique of obtaining the cultures, about four years ago we had an epidemic in which nearly 60 per cent. of the babies had a catarrhal condition of the upper respiratory tract, and the pathologist told us we could not get the cultures from the nasal secretion, but must get them directly from the blood. Finally I found a practical way of getting the blood. The temporal vein, when the baby cries, is very prominent, and on inserting a needle the desired blood can readily be obtained.

We begin to respect influenza a great deal more as we understand it better. I had an interesting experience this last winter. I was called to see a case of meningitis. The child had received a fall on Saturday, and on Monday had meningitis. On lumbar puncture, pure culture of the influenza bacillus was obtained. Perhaps such cases are more frequent than we have thought. Dr. Adams, Dr. Koplik and Dr. LaFétra have reported cases of influenza meningitis.

DR. CHURCHILL.—Dr. Shaw's remarks concerning the case of influenzal meningitis reminds me of a case I saw this last winter—one of twins, one week old. Both were taken sick suddenly, and when I saw them there were absolutely no signs typical of meningitis. I could not find anything to account for the condition, although cultures were made from the throat. The first baby did not have any characteristic symptoms until twenty-four hours before death. At postmortem we found purulent meningitis. We were able to obtain the postmortem by holding out to the parents the hope of throwing some light upon the second case. Cultures showed pure culture of influenza bacillus. The second baby also died, but in this case autopsy was declined.

Dr. Shaw says that at last we are coming to respect influenza.

I have respected it for a long time. I do not allow my children to go into a house where there is a case of influenza. It is a very serious disease. It attacks almost all the tissues of the body. I think we shall come to speak of influenzal bronchitis, influenzal pneumonia, influenzal meningitis, etc. I do not understand why it is that we do not find the organism in the ear.

DR. KERLEY.—I am in accord with what Dr. La Fétra has said and Dr. Holt intimated in his paper, in relation to operation upon the mastoid when fever is the only symptom. I am sure I have seen mastoids opened where it was not justified. I take the attitude that the aurist must have definite symptoms and not consider the matter of temperature at all. I will not have my cases operated upon unless there is prolapse of the posterior meatal wall or other evidence of the disease aside from temperature.

DR. HOLT (closing remarks).—I do not believe that Dr. Koplik would take the ground that where ignorance is bliss 'tis folly to be wise. I took this matter up with the hope that we might find some way to eliminate the great number of house infections.

As to the blood cultures during life, we were discouraged from making them by the postmortem findings. If the influenza bacillus was not found in the blood in fatal cases there was little hope of getting it during life.

One of the reasons for taking up this subject was to do something to prevent the great number of mastoid operations done with no other indication than that of temperature continuing after ears had been opened or had opened spontaneously. As has been said, there should be definite local indications for this operation, not simply unexplained high temperature.

As regards the blood count, most of the children had a moderate leukocytosis. There were comparatively few of the long-standing cases that did not get up some local inflammation, and it was from that the leukocytosis arose. There were, however, many cases in which fever was the only symptom for a considerable time.

In the cases complicated by pneumonia, that condition appeared usually as a consequence of the infection and not the primary disease.

I believe we are warranted in taking the position that there is a definite disease—influenza. We should separate certain definite clinical forms of the disease. I hope other members will take up the question and that in time we may be able to settle the question of how inclusive influenza really is.

Biological
Medical
Ls

RJ
1
A6
v. 21
Biological
& Medical
Serials

American Pediatric Society
Transactions

PLEASE DO NOT REMOVE
CARDS OR SLIPS FROM THIS POCKET

UNIVERSITY OF TORONTO LIBRARY

STORAGE